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# ATTENTION AND THORACIC BREATHING.

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This paper contains a statistical study of data experimentally obtained in regard to the correlation between changes in thoracic breathing and changes in attention. In calling the study "statistical," the writer has two characteristics in mind. In the first place, the results presented consist of records taken from many experimental subjects, a few to each subject, and not of records taken from a few well-trained observers who served as subjects repeatedly. In the second place, the control of the experimental conditions was not so secure as to enable the ex-

[1] The writer was assisted in an important, though minor degree by the following students: L. J. Boynton, H. S. Wheeler, and M. B. Woodbury, graduates; and E. M. Bennett, Maud Dewar, Lillian Dixon, A. S. Carlisle, M. E. Cooke, G. C. Hanna, M. C. Hunt, Frances Warren, and Edith Young, seniors. The writer supervised the experiments, served as one of the subjects in Groups 1 and 4, and compiled all the tables which appear in this paper. The students named made the experiments, computed the breathing-rates upon which Tables 2 and 3 are based, and inspected the individual kymograph-tracings to determine the changes in amplitude and expiratory pause upon which Tables 1 and 4 are based. The results as to rate obtained by each different set of student experimenters have been averaged separately by the writer to ascertain the presence of any divergence which might unfit them for being averaged together. No such divergence exists. The organ music of Group 3 was furnished by Dr. Hamilton C. Macdougall, professor of music at Wellesley College. The experiments of this group, considered in another connection, constitute part of an investigation now pursued by Professor Macdougall with the assistance of the writer. The investigation here reported extends from the January of 1900 to the spring of 1904.

perimenters to dream that variations in the results would fall under the scientific "law of error." The experimental condition is the nature of the subject's attention, high and steady as in arithmetical computation, high and unstable as in keen annoyance, low and wavering as in moments of complete idleness, or whatever it may be. The experimental results are such changes in his thoracic breathing as may occur *pari passu* with changes in the degree and steadiness of his attention. As regards the experimental condition, the experimenters merely assume that a given method of attracting and holding the attention—as, for example, a sum dictated in mental arithmetic —produced much the same kind and degree of attention in the *majority* of cases. As regards the experimental results, the compiler of this report claims simply that they manifest certain *tendencies* in the majority of subjects to breathe in a given manner under a given condition, precisely as statistics in regard to phthisis show a greater *tendency* to this disease in state-prisons than among the general population.[1]

The investigation was suggested by the paper of Professor James R. Angell and Dr. Helen B. Thompson upon the correlation of changes in respiration and circulation with changes in consciousness.[2] These investigations made "two very complete series of tests on two different subjects," and "less extended observations upon a number of different subjects." The special object of the investigation here reported was to obtain such evidence for or against the Angell-Thompson conclusions as might be obtained from the massed results of tests of thoracic breathing taken from a large number of young persons. The subjects included in round numbers one hundred and twenty college girls of no profound psychological information and fifty boys and girls between the ages of four and eleven.

For purposes of comparison two other sets of records are introduced, representing on the one hand, greater introspective ability, and on the other, lower conscious development. These records include (1) the results of tests made upon the writer and a senior-student, who shared in the investigation, during genuinely absorbing games of duplicate whist played with keen and accustomed rivals, and (2) the results of somewhat fragmentary tests upon two frightened cocker spaniels.

As a supplement to other studies, the special value of the evidence here presented consists precisely in the facts (1) that the tests were made upon so many different subjects a few to each subject, and (2) that so large a number of the subjects

[1] On this use of the word "statistical," compare Ebbinghaus, Ueber das Gedächtniss, pp. 18-20.

[2] "Organic Processes and Consciousness, *Psychological Review*, 1899, pp. 32-69.

were mentally immature. The advantage of experimenting upon untrained subjects, whether children or adults, is the following: It is true not only that respiration, unlike circulatory phenomena, is largely under voluntary control, but also that in a pneumographic test the attention of the subject is necessarily called to his breathing by the nature and position of the apparatus. There is, therefore, great danger both that the subject of scientific training will breathe according to theoretical considerations—or will try not to do so, which is quite as bad—and that the alert adult who is tested many times over will breathe according to past self-observations. This risk is avoided when average or when typical results from a large number of untrained subjects are secured. An additional advantage in the case of children is the fact (recognized by Binet) that they may be roused to very real emotions, even under laboratory conditions. In this investigation, genuine emotion was very rarely secured except in the case of the children, the whist-players, and the dogs.

On the other hand, and as against the force of the main body of our evidence, these two considerations must be admitted: first, that the child cannot check by any introspective accounts of his own, the experimenter's assumptions as to his experiences under the varied experimental conditions, and second, that any subject who is tested only twice, for scarcely more than half an hour at a time, will never rid himself of a certain self-conscious curiosity in regard to the experiment. He will not, indeed, of set purpose adopt or avoid a certain manner of breathing, but he will never breathe normally in the sense of breathing as he would under the various conditions minus the tell-tale apparatus. It is probable that one does not often secure records of really normal breathing even from the much-tested subject; it is certain that one never does so from the raw recruit. In full recognition of these considerations, these results are presented as a supplement to other studies—a supplement which has most decidedly the defects of its virtues.

As already stated the primary purpose of the investigation here reported was to verify so much of the conclusion of Professors Angell and Thompson as refers to respiration. The exact scope of the study may be indicated by noting the nature of this conclusion and by enumerating, on the one hand, the changes in attention and, on the other, the changes in breathing which have been taken into account. The two sets of changes, if one may reiterate, stand to each other as experimental conditions stand to experimental results. In connection with the statement of attention-changes, it will be convenient to outline the expedients used to secure them; and in connection with the statement of breathing-changes, to describe the apparatus em-

ployed in registering them and the method by which the statistics were compiled from the graphic records.

In the following paragraph Professors Angell and Thompson formally state their thesis in regard to the correlation between circulation and respiration on the one hand, and consciousness on the other: "When the attentive process runs smoothly and uninterruptedly, these bodily activities progress with rhythmic regularity. Relatively tense, strained attention is generally characterized by more vigorous bodily accompaniments than is low-level, gentle, and relatively relaxed attention (of drowsiness, for instance); but both agree, so long as their progress is free and unimpeded, in relative regularity of bodily functions. Breaks, shocks, and mal-coördinations of attention are accompanied by sudden spasmodic changes and irregularities in bodily processes, the amount and violence of such changes being roughly proportional to the intensity of the experience." In constituting a "hierarchy" of psychological conditions as representing "increasing stability of attention," the authors put at one end of the scale, the profound emotions, and at the other, sustained attention to continuous sensations, and mental application (such as mental calculation under favorable conditions), and many cases of non-emotional revery, such as the revery of drowsiness.

In guarding their position, the authors write: "It should not be understood that we dogmatically deny any constancy of changes apart from the form of constancy we emphasize, although such constant conditions are rare. We simply maintain that from our observations the only feature which appears essentially constant under *all psychological* conditions is the relative stability or instability (of the dynamic type) which these organic activities manifest in connection with the different processes of attention."

Briefly, then, the conclusion to be tested is this: The correlation between regular breathing and stable attention and between irregular breathing and unstable attention, is the only essentially constant correspondence between respiration and consciousness. In view of the history of opinion upon the point in question, it will be necessary in verifying this thesis, to show not only that (1) the correlation stated is "essentially" constant but also (2) that there is a much less constant correspondence between opposite features of breathing and either (a) high-level and low-level attention or (b) antithetical affective states.[1] The correlation between a change of breathing and a

[1] *Cf.* Angell and Thompson, *op cit.*, last sentence on p. 44 and second note on p. 45. An excellent summary of results previously obtained in regard to the relation between organic processes and consciousness is given by these writers.

change of attention may perhaps be considered as essentially constant if the breathing-change occurs in a goodly majority of the cases in which the attention-change is scheduled to occur. A large number of negative instances can always be explained on the supposition that the means taken to control the attention failed of doing so.

The procedure here reported was scaffolded upon the cross-division of attention as stable and unstable, high-level and low-level. In other words, the variations sought were two-fold: (1) variations in stability and (2) variations in degree. An attempt was made systematically to secure for comparison cases (1) of low and unstable attention, (2) of low and stable attention, (3) of high and unstable attention, and (4) of high and stable attention. Expedients to secure high-level and unstable attention were divided between attempts to secure pleasurable and attempts to secure unpleasant excitement.

Before detailing the expedients employed to secure these varieties of attention, it seems desirable to define more exactly the terms "stability" and "degree" (or "level") as here applied to the attention. By the writer, attention is understood to be great in degree or high in level when there is a maximal difference in clearness between the "focal" and the "marginal" contents of consciousness. It is understood to be stable when each focalized content gradually gives place to the next without sudden blurring or abrupt transition. Great stability and a great degree of attention do not necessarily go together although attention can be both great and stable; as it is, for example, in rapt attention to music or in unhesitating literary composition. In fact, attention which is great in degree tends, because it offers strong resistance to distraction, to be stable also. In virtue of its interest, primary or acquired, the train of ideas attended-to not only stands in clear relief but persists. Nevertheless, in an absence of distraction, attention may be *stable without being great*, as for instance, in reading "David Copperfield" for the tenth time. Moreover, in strong emotion, attention may be *great without being stable*. In violent anger, for example, there is a kaleidoscopic shifting of various phases of the exasperating situation even while all ideas irrelevant to this situation are blurred or inhibited. This high-level but unstable attention cannot indeed exist except in emotional excitement since only by the competition of high affective values in various phases of the situation can resistance to distraction be so overcome as to allow this oscillation. On the other hand, high-level attention if unstable, must be more unstable than low-level attention can ever be, since the higher the level, and the sharper the focus of the content attended-to, the wider must be the swing, the more violent the shift, which brings

any other content into prominence or focus. For illustration, compare the catastrophic changes of rage, in which one is fully occupied now in imaging the consequences of the offence, now in picturing the offender, and now in enduring one's own suffocating chest and throat sensations, with the easy shifts of attention by which in walking along the street on a March afternoon one notes first the coldness of the wind, then the odor of hyacinths from a florist's shop, then the cakes in a baker's window, and then the strains of a hurdy-gurdy. In the latter case, the content-transitions are as abrupt as possible, but consciousness does not seem at one instant all cakes and at the next all hurdy-gurdy.

Attention which is both great in degree and also highly stable,—such attention, for example, as one gives to the cards which are played in an exciting game of whist—usually involves that complex of organic sensations which we know as the "strain" or "effort" consciousness. In really stable attention, consciousness is simply tinged or flavored by this feeling of effort. There are breaks in the stability of attention if this feeling ever and anon comes into focus. When one is trying to attend to a conscious content which is extremely uninteresting, the focus will shift between this content, the strain-consciousness, and the motive for attending. But to attend to being attentive to anything is, *ipso facto*, to be inattentive to that thing. When the effort-consciousness comes into focus, the mechanism which produced it by keeping attention stable at a high level has temporarily broken down. On the other hand, a sharp fall in the level of attention as distinguished from a temporary break in its stability, is ordinarily characterized by that complex of organic sensations which we know as the "relaxation" or "relief" consciousness.

In the understanding of the writer the distinction just made between the stability and the degree or level of attention tallies with the distinction indicated by Angell and Thompson in language which has a somewhat biological ring.[1] Moreover, it would seem that the distinction between high-level and low-level attention is almost if not quite tantamount to the antithesis made by Binet and his co-workers between mental activity and mental passivity, "entre les excitations quelconque et l'état de repos physique et moral."[2]

[1] See especially the last paragraph of their paper, p. 69.

[2] The only difficulty in this *rapprochement*, is in the classification of depressing emotion. Binet and Courtier recognize the existence of this type of emotion in their classical treatment of "La Vie Émotionnelle" (*Année Psych.*, 1896, pp. 80-82), although they had not obtained any instances for experimental purposes. This depressing emotion is antithetical to exciting emotion. But is attention in depressing emo-

An account may now be given of the experimental procedure in this investigation. In the first two of the five groups into which the experiments fall, an attempt was made to secure from every subject each of the four varieties of attention named above. In Group 1, the subjects were ninety-three college students and the writer; in Group 2, fifty children. In the following outline of experiments a device to control the attention will for convenience be called a "stimulus." To secure low-level attention no stimlus was necessary. *Low and unstable attention* is one's normal condition, and breathing-records taken during this state constitute the standard of comparison for other records. This condition was supposed to be secured when the subject sat idle, paying casual attention to the movements of the experimenters and too much affected by the novelty of the situation or by recent distraction to fall into revery.

On the other hand, cases of *low-level but stable attention* were assumed to be constituted by cases of apparently non-emotional revery. Cases of genuine revery were actually secured only from the students. For a reason which will later appear, cases in which the children sat unstimulated but absorbed must be construed as cases of high-level attention. In the students, the degree of stability in revery must have varied with the absence of distraction. In most cases, the level of attention was probably lower than when the subject sat idle but alert. The subjects were directed to "try to go to sleep."

The stimulus designed to secure *great and stable attention* was, in the case of the students, either (1) the dictation of a sum in mental arithmetic, at such a rate as to avoid both "unoccupied leisure" and "insufficient time for completing"[1] the operations, or (2) the dictation of a series of nonsense syllables, or (3) the dictation of a jingle or of a bit of poetry to be memorized, or (4) the reading of a scientific passage to be summarized. With the children this stimulus was always some form of mental calculation ranging from a simple sum in arithmetic to the counting of a few objects drawn on the blackboard.

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tion at a low or a high level? It is altogether impossible to answer this question introspectively. One may suggest, however, that "prostrating fear" and "deathly" disgust are likely to end in unconsciousness and that in incipient faintness attention is at a level anything but high. It may be added that attention-stability of the "abnormal paralytic type" (recognized by Angell and Thompson as an exception to the instability of attention in emotion) does not necessarily correspond with emotion which is depressing in Binet's sense of lowering the vital functions. Such a correspondence has yet to be proved. It should be expressly noted that the word "excitement" is not used in this paper in the physiological sense (Binet's) but as the popular equivalent for attention which is great but unstable.

[1] Angell and Thompson, *op. cit.*, p. 49.

For the most part the stimuli designed to secure from the students *great but unstable attention*, or in other words, emotional excitement, consisted in the reading aloud of certain passages from books. In many of the cases in which the stimulus was reading, an attempt was made to secure the subject's attention before the graphic record was begun. The experimenter, for example, would read in continuity for some moments beforehand or would summarize the context as vividly as possible. Nevertheless, the attempt to secure excitement by reading was a failure. Records taken during the reading of pleasurably interesting passages have been credited entirely to the score of fairly great and fairly stable attention. By reading the most distressing or revolting passages which could be used with propriety, unpleasant emotion, usually disgust, was undoubtedly secured in many cases. Nevertheless, the behavior and testimony of the subjects and the nature of the results all contribute to the conclusion that the majority of the subjects paid steady attention in spite of qualms of faintness or of the impulse to "get up and go away."

Apart from the reading, some instances of superficial emotion (Binet's *emotions psychiques volontaires*) were procured by asking the subject to "think about" the "pleasantest" or "most unpleasant" thing that had happened to her within a month. These affective reactions were cut across by the effort to recollect and were almost too feeble to be taken into account. In addition, some cases of unpleasant excitement were secured by threatening to "hurt" the subject and then actually giving her a smart prick or pinch. This expedient should have been used in a larger number of cases. A few cases of mild excitement were secured with such smells as asafœtida and pyridin. Agreeable smells were also tried but the results were altogether equivocal. Indeed, smells are obviously unsuitable stimuli on account of their direct sensorial-reflex effects upon the breathing.

With the children also, the attempt to secure pleasurable excitement was a failure, but with them the attempt to secure unpleasant excitement succeeded. Pleasure, far more genuine than was ever secured in the students, was indeed obtained by telling fairy-stories or by drawing with running comment grotesque figures of "horses" and of "lions" in bright colors on the blackboard. The small subjects, however, betrayed no excitement but rather a "riveted attention," nor can the results of the experiment be interpreted except on the supposition that the attention was extremely stable. At this point it should be noted that the breathing of the children when they were allowed for a few minutes to sit unmolested and look about them, became toward the end of this period exactly like

their breathing when they were purposely amused by the experimenters.[1] These cases were at first labeled "revery." It is obvious, however, that in them we have to do not with low-level attention but with the high-level attention involved in a keen interest in the movements of new and attractive acquaintances and in novel surroundings which had ceased to be alarming. Notwithstanding the failure of the attempt to secure pleasurable excitement, strong annoyance was actually secured (1) by pinching a finger slightly with a very formidable looking nut-cracker, or (2) by administering some bad smell (pyridin, usually), or (3) by putting drops of a grimy and sticky liquid on the hands of the girls or giving the boys, without warning, a kiss.

Thus as the actual fruitage of the first two groups of experiments, we have the following sets or sub-groups of cases or observations: first, from the students and children alike, a group of cases of normal, low-level and unstable attention, and of the high-level and stable attention involved in "mental work;" second, from the children, two groups of cases of the high-level and stable attention involved in being agreeably entertained, and a group of cases of the very great and very unstable attention involved in being sharply annoyed; third, from the students, a group of cases of the low-level but fairly stable attention involved in non-emotional revery, a group of cases of the high-level and stable attention involved in listening to pleasant reading, and a group of cases of the high-level attention with very marked individual variations in stability which is involved in listening to extremely unpleasant reading.[2] The few cases of voluntary emotions and of annoyance secured from the students by the threat and infliction of pain are not numerous enough to count as statistical groups.

In the case of the students and of the older children, attention no doubt reached its maximum of stability in the mental arithmetic. In such enforced mental application, however, there was probably as a rule no real affective equilibrium, but rather the slight unpleasantness of the strain-consciousness. In the younger children, on the contrary, attention probably reached its maximum of stability not in the task-work, about which they cared relatively little, but in the pleasurable entertainment furnished by the experimenters.

The experiments of Group 3 supplement those of Groups 1 and 2 by furnishing an additional number of instances of great

[1] See page 278 below.

[2] Even at the time these experiments were made it would have been a delicate operation to divide this set of cases into sections according to the fixity of the attention secured; at the time this paper is written, the task has become impossible.

and stable attention. The stimulus was organ-music. The subjects were twenty students. These three groups of experiments made upon many different unpracticed subjects constitute the nucleus of the investigation.

As already indicated, two other sets of data have been introduced for purposes of comparison with the main body of results. The fourth group includes the whist-tests made upon two of the investigators. The value of these experiments consists in the fact that in addition to some instances of high-level and stable attention, they offer many instances of the unstable attention implied by marked affective disturbance. Here pleasure, genuine though fleeting, was actually obtained. Thus these experiments supplement to some extent the defects of the first two groups. Moreover, we have here our best instances of a sharp and sudden fall in the attention-level with the feeling of relief which is involved in such a drop.

With the dog chiefly employed, "P.," the first records taken represent great but unstable attention, since the animal, an exceptionally nervous creature, was frightened by the apparatus, especially by the hum of the electric motor. As, however, toward the end of each sitting she relapsed into a drowsy repose in the lap of her owner, a certain number of records may be regarded as representing a fall in the attention-level and an increase in stability. The other dog, "A.," a healthier specimen, was not frightened by the apparatus after the first few moments, and was used chiefly in experiments to control the breathing-rates obtained from P. and to find a method of securing high-level and stable attention without disturbing the apparatus. So far every expedient used to attract her attention (soft whistling, the rattling of a leading-chain and so on) has produced movements of the whole body.[1]

There is one more point which it is important to note before turning from the psychological to the physiological phase of the experiments. In the first and third groups, some attempt was made at introspective control in spite of the fact that the experimenters wished the subjects to know as little as possible about the purpose of the investigation. At the end of an idleness-record the subject was desired to write down in detail what she had "thought about," and at the end of a reading or music experiment whether or not she had "listened" and how she had "liked it." From the children, of course, no such introspective check could be obtained. Yet the expression of their faces, and especially the flush produced by annoyance, served

[1] The cocker P. was in her third year when these experiments were made and weighed only thirteen pounds. A. was seventeen months old and weighed eighteen pounds.

almost as well. Nevertheless, there must remain unrevealed by the checks of introspection or behavior a very wide variation in the attention-conditions produced in different individuals by the same stimulus. Only extreme variations could be detected and only the records corresponding to them thrown out.

A few more details may, perhaps, be suggestive for demonstration experiments. At the beginning of an arithmetic or memory experiment the subject was told that she would be asked to give the answer to a sum or to recite something which would be dictated to her or to summarize something which would be read to her. She was asked, however, not to speak until she was directed to do so. The material to be memorized verbatim was read over twice with an interval of about thirty seconds between the readings. The subject was told beforehand that this would be done and was directed to recall what she could in the interval and be ready to supplement her first impression by the second reading. The following is a sample of the sums used with the students: $5 + 8 + 2 \div 3 + 15 \div 4 \times 10 \times 2 \div 50 \times 10 \div 5 \times 10 \div 5 + 8 \div 4 \times 12 \div 6 \times 2 + 4 \div 2 \times 100 \div 500$. This sort of arithmetic is, of course, incomparably easier for a bad visualizer than is the multiplication of one three-place number or even of one two-place number by another. It was believed, however, that if the task were difficult many of the subjects employed would not try to do it at all. The series of nonsense-syllables was: Raj, feg, nif, wud, fev, rem, nij, fum, hiv, tig. The "poetry" varied between Milton's "Sonnet on Blindness" and Stevenson's "Birdie with a Yellow Bill." The passage most frequently read for summarizing was Ayrton's "Practical Electricity" the first sentence of page 1 and page 2, entire. The passage most successful in sustaining amused attention was pp. 39 and 40 of the "April Baby's Book of Tunes" (by Elizabeth of the German Garden), that is, the clearing up scene after the party. The passages which produced the strongest unpleasantness were (1) the description of the plague burial-pit from "Rienzi," and (2) a part of "The Cone" in H. G. Wells's "Thirty Strange Stories," viz. pp. 312-320.

The organ music was selected for a purpose extraneous to this investigation. It was of three sorts (1) hymn-tunes, such as the familiar "Vox Dilecti," (2) two rather dismal chorals from Mendelssohn, and (3) elaborate compositions ranging from Rubenstein's "Torch-Light Dance of the Brides of Cashmere" to the Dead March in "Saul."

The procedure in the whist-experiments was as follows: The eight duplicate-whist boards were played through once before any records were taken in order that the attention of the subject might be completely absorbed in the game. In other words, records were taken only in connection with the duplicate round. The score was read at the end of each hand. The experiment required the help of three persons besides the four players. The first assistant marked on the graphic record the letters and numbers called out by the other two. The second assistant called the numbers of the hands and of the tricks as they fell, noting on a record sheet of her own whether the subject's side took the trick or not. The third assistant was always a person who had played often enough with the subject to make pretty shrewd inferences in regard to her mental processes under given circumstances. This assistant sat where she could both see the subject's hand and watch all the plays. When she made some inference in regard to the subject's state of mind, she indexed her note with a letter which she called to the first assistant who marked it on the graph. When a hand was played out, the third assistant checked doubtful in-

ferences by reference to the subject. In general, however, the subject was left to enjoy her game unmolested by requests for introspection. The whole value of the method depends on the fact that both subjects took the game seriously. Its glaring defect is in the fact that the third assistant could not possibly take account of the complexity of the subject's feelings. Often, for example, in spite of incidental triumphs or misfortunes, a tendency toward anxiety or exhilaration would run through the playing of a whole hand or succession of hands.

It is now in order to outline the respiratory changes studied. These are (1) changes in regularity, (2) changes in rate, (3) changes in the depth or amplitude of the thoracic phase of the respiration, and (4) changes in the length of the expiratory pause. The records have never been worked over to determine changes in the rapidity of the respiratory act itself or in the relative rapidity of inspiration and expiration. Moreover, the attempt at recording changes in the depth of abdominal breathing side by side with changes in the depth of thoracic breathing was abandoned simply because the manipulation of two tambours and the study of two sets of tracings required more time than the experimenters[1] could give to the investigation.

The pneumograph employed was the Sumner belt. The kymograph revolves upon a horizontal axis and is six inches in diameter and eighteen inches long. During most of the experiments, the revolution-time was about four minutes. During the music experiments, the last group to be performed (although, on logical considerations, it is numbered three), this time was about three and one-third minutes. The time-marker was a magnet-pen (Sumner's) which registers seconds on the kymograph side by side with each pneumograph tracing. In all the experiments, except in those of Group 3 and a part of Group 5, the writing arm of the tambour was tipped with a pointed strip of manila paper and measured about six inches; in these later experiments the lever was less than three inches long and was tipped with a fine straw. A single sheet of smoked paper held cross-wise from six to seven pneumograph-tracings each with its parallel time-record. The instruments were not moved in the horizontal plane with the aid of a carriage but were clamped to a horizontal rod in such wise that although they could easily be raised or lowered, they did not slide in a line parallel to the drum. After practice, however, the experimenters succeeded in moving the whole standard and readjusting the writing points so quickly that a twenty-minute record could be secured virtually unbroken.

The pneumograph belt used with the dogs was made to order on the model of his other belts by Mr. Horace E. Sumner,[2] of the Harvard

[1] See note, page 261.

[2] A drawing of the Sumner pneumograph may be found in Titchener's "Experimental Psychology," Inst. Man. Qual., p. 184.

Physiological Laboratory. It was designed for use with any animal in size between a cat and a thirty pound dog. It is eight inches long without stretching, is made of lighter weight rubber than the ordinary belt and has a more flexible spring. A belt of this size, if desired for use with animals as small as a fifteen pound dog, should not have the flat metal braces at the ends; these pieces fail to serve their purpose and they annoy the animal.

The kymograph is turned by an alternating-current motor and the speed is reduced by a system of pulley-wheels. The change of speed between the earlier and the later experiments was due to a permanent change in the light current utilized. In the earlier experiments the circuit of the magnet-pen was made and broken by Lough's electrically actuated pendulum; in the later experiments by a mercury-contact metronome (Maelzel's).

The breathing-rate per minute was computed with the aid of the second marks for every tracing, long or short, which had separate significance. Table 3 is made up of averages of rates so determined. On the other hand, changes in form as distinct from the rate of breathing, that is changes in regularity, changes in the depth of respiration, and changes in the length of the expiratory pause, were determined not by any system of count or measurement but simply by the careful inspection of two experimenters who verified each other's conclusions. A numerical character is introduced into these data only by the count of cases or "observations." An attempt at genuine measurement with triangle and rule in experiments at once so extensive and so rough would be a glaring instance of misplaced accuracy.

In the first two groups of experiments, as a standard of reference for determining the changes which took place in the rate and form of breathing during an attention-stimulus, a normal tracing was taken before each separate stimulus and this tracing ran into the stimulus-tracing on the kymograph without fresh adjustment of the apparatus. In these experiments, therefore, changes in the form and rate of breathing during stimulation mean changes from the form of the normal preliminary tracing taken in every case. Each stimulus-record was also followed by a tracing taken under conditions which were normal except for the possible after-effects of the stimulus. Breathing-rates determined from these tracings are called "reaction-rates" or "relief-rates." Little use has been made of the reaction-tracings in determining the normal form of breathing. In the "revery-tracings" of the first two groups of experiments, and in all the tracings of the last three groups, modifications of the normal form and rate of breathing have been determined much less accurately and satisfactorily. In the revery-tracings, in which no stimulus was given, changes of form were determined by comparing the second half of the tracing with the first. The character of the average rate was

determined by comparison with the average normal rates of the students or children. In the music experiments in which many of the stimuli lasted over fifteen minutes, the standards of reference are normal tracings taken at the beginning of each sitting. Two or three music-tracings have but one such standard. The reason for this unsatisfactory procedure was the desire to minimize the subject's self-consciousness and thus to get as much æsthetic effect out of the music as possible. In the whist experiments, the standards are still less satisfactory since they consist simply of groups of tracings taken at about the same time as the stimulus-tracings, but at different sittings. In the tracings obtained from the dog P., changes are always in the direction of more rather than less normal breathing. None of the tracings were assumed to constitute a standard of reference. The whole mass of tracings was simply considered together to determine the modifications of breathing which took place as fear subsided. The tracings obtained from A., have not been studied with reference to changes in form.

Before passing to the presentation of results it remains only to note the massing of individual results. Except for the experiments on the whist players and the dogs, the results of whole groups of subjects are counted together as instances of the different varieties of attention. When an individual furnished more than one instance of a given variety of attention, these observations are counted each as one, quite as if they had been furnished by different subjects. For example, the average breathing rate for the students under normal conditions, that is, conditions of low and unstable attention, is obtained from 505 observations furnished by 104 different individuals. The justification of this procedure is the fact that the results from unpracticed subjects vary as widely for the same individual as for different individuals. As regards variation in rate, results from individual subjects have been averaged separately a sufficient number of times to offer ample illustration[1]

[1] In illustration of this statement, the following figures may be taken from the music experiments. All subjects from whom over fifteen normal tracings were taken are included. The tracings cover periods from 30 seconds to 3½ minutes in length. The rates are computed per minute.

| | | | | |
|---|---|---|---|---|
| All tracings of rate faster than average normal. | Cases | 164 | Av. rate 20.7 | M. V. 1.5 |
| All tracings of rate slower than average normal. | | 184 | 14.1 | 2.2 |
| Subject C. | | 24 | 12.8 | 2.0 |
| " D. | | 17 | 18.6 | 3.2 |
| " N. | | 18 | 15.1 | 1.4 |
| " P. | | 27 | 20.3 | 3.7 |
| " S. | | 23 | 19.8 | .9 |
| " T. | | 27 | 18.2 | 1.3 |

of this statement. As regards form, the assertion cannot be proved without photographs of the tracings.

The writer has now attempted to state the specific purpose and point of departure in this investigation of the correlation of changes in breathing with changes in attention, to outline the changes in attention which have been taken into account with the expedients used to control them, to enumerate the changes in breathing which have been studied, and to indicate the apparatus used in registering them and the method by which they have been given numerical expression. The conclusions will now, for the convenience of the reader, first be tabulated and afterwards be taken up in detail with such numerical evidence as may exist for them.

The following conclusions may be drawn:

I. In regard to the form of breathing:

1. When the stability of attention increases
   (1) The length of the expiratory pause tends to increase in regularity, but
   (2) The depth of breathing does not tend to increase in regularity under all the experimental conditions.
2. When the stability of attention decreases, both expiratory pause and depth of breathing decrease in regularity.
3. When the degree or level of attention rises
   (1) The expiratory pause tends to decrease.
   Exception: If attention is strongly expectant, the expiratory pause tends to increase.
   (2) Breathing tends to become shallower.
   Exceptions: In distinct pleasantness breathing tends to become deeper.
   In extreme animal fear breathing becomes deeper.
4. When the degree or level of attention falls either below the normal or after conscious strain
   (1) The expiratory pause becomes shorter than the normal.
   (2) Breathing becomes deeper than the normal.

II. In regard to the rate of breathing:

1. When the level of attention rises
   (1) Breathing which before the stimulus has been slower than the average normal tends to be accelerated.
   (2) The rate of breathing which has been faster than the average normal is much less affected than the rate of slow breathing and may even be retarded.
   Exception: In the case of very stable attention with the consciousness of strain (mental application), breathing, whatever its rate before the stimulus, tends to become decidedly faster.

2. When the level of attention falls either below the normal or sharply after great strain, breathing tends to become faster as one of the symptoms of relaxation.
3. When the stimulus comes to an end, breathing tends, unless the affective disturbance persists, to return to the rate which it exhibited before stimulation.

By far the most constant of these changes is the acceleration of slow breathing with a rise in the attention-level. Perhaps the next most constant change is the variation in the regularity of the length of the expiratory pause with variation in the stability of attention. Whether any of the changes deserve the adjective "constant," the reader must decide for himself from the figures. The presentation of this numerical evidence is now in order.

Tables 1 and 2 present all the statistics obtained from the first two groups of experiments in regard to changes respectively in the form and in the rate of breathing with changes in the nature of attention. Table 3 is made up of averages of rate-counts for tracings taken under different conditions of attention in the first two groups of experiments. The results of the third group of experiments are excluded from the tables because the standards of normal breathing obtained in this group are unsatisfactory in determining changes under stimulation. The results of the fourth group are excluded for the same reason and also because they are of an individual character and thus not comparable with the massed results of many different subjects. It should be noted in passing that nearly all the observations in each set of cases in the tables were obtained from different individuals. However, virtually all the cases of memorizing were obtained from subjects who also furnished cases of arithmetic, and more than one kind of memorizing was often required of the same subject. Out of the 154 cases of mental application in the students, 78 were cases of arithmetic and 76 cases of memorizing.

It will be convenient in discussing the conclusions summarized above to state severally in connection with the various inferences which may be drawn from the tables such contradictory or corroborative evidence as may be drawn from the results not included in the tables. Changes in form will be discussed before the changes in rate. Thus the first data discussed will be those of Table 1.

In view of the account of experimental conditions already given the only column of this table which seems now to require explanation is the column headed "Number of Observations." In the first group of experiments, the later observations of high-level attention have been singled out from the total number and represented separately. This was done because they

TABLE I.

*Changes in the Form of Breathing with Changes in the Degree and Stability of Attention.*

| Group of Experiments. | Change of Attention. | | | Number of Observations. | | Changes in Regularity of | | | | Changes in Amount of | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Condition. | Nature. | | | | Breathing Amplitude. | | Expiratory Pause. | | Breathing Amplitude. | | Expiratory Pause. | |
| | | In Degree. | In Stability. | | | Increase Cases % | Decrease Cases % | Increase Cases % | Decrease Cases % | Increase Cases % | Decrease Cases % | Increase Cases % | Decrease Cases % |
| 1 Subjects: Students. | Mental arithmetic and memorizing. | Increase | Great increase | Later | 43 | 65.1 | 34.9 | 65.1 | 30.2 | 27.9 | 72.1 | —— | 93.0 |
| | | | | Total | 154 | 22.7 | 17.5 | 42.2 | 14.9 | 15.6 | 33.1 | 8.4 | 53.2 |
| | Listening to pleasant reading. | Increase | Increase | Later | 19 | 31.6 | 52.6 | 36.8 | 42.1 | 21.1 | 68.4 | 52.6 | 26.3 |
| | | | | Total | 39 | 20.5 | 33.3 | 28.2 | 20.5 | 28.2 | 43.6 | 41.0 | 33.3 |
| | Listening to unpleasant reading. | Increase | Increase (Great variations.) | Later | 27 | 37.0 | 55.6 | 59.3 | 40.7 | 22.2 | 70.4 | 18.5 | 81.5 |
| | | | | Total | 52 | 40.4 | 34.6 | 38.5 | 32.7 | 26.9 | 51.9 | 34.6 | 48.1 |
| | | Decrease | Increase | Total | 45 | 24.4 | 71.1 | 51.1 | 33.3 | 73.3 | 22.2 | 22.2 | 62.2 |
| 2 Subjects: Children. | Mental arithmetic, | Increase | Increase | | 49 | 44.9 | 46.9 | 65.3 | 32.7 | 34.7 | 63.3 | 61.2 | 38.8 |
| | Entertainment, | Increase | Great increase | | 59 | 57.6 | 39.0 | 79.7 | 15.3 | 72.9 | 25.4 | 61.0 | 39.0 |
| | Annoyance, | Increase | Decrease | | 52 | 21.2 | 78.8 | 25.0 | 67.3 | 38.5 | 61.5 | 46.2 | 51.9 |
| | Revery so-called= entertainment. | Increase | Increase | | 51 | 56.9 | 39.2 | 78.4 | 15.7 | 78.4 | 21.6 | 62.7 | 33.3 |

were made at the same period and by precisely the same methods as the observations of revery in the students and of all the attention-changes in the children. On the other hand, the earlier observations of high-level attention in the students were made earliest in the course of this study. But in working over the earliest tracings, relatively inconspicuous changes in form were left out of account. If, therefore, the figures for the total number of observations of great attention in the students, that is, the totals printed in heavy-faced type, be compared with the other totals in the table, the face-value of the figures would indicate that the breathing-changes in question were more common in the children than in their elders, and more common when the students fell into revery than when their attention changed in other ways. Neither of these indications is warranted by the facts. Only the figures in the lighter type may be compared with one another if the point in question is the number of times the breathing changed *in any way* in depth or regularity or expiratory pause with a given change in attention. It should also be noted in passing that the stimuli first employed were less effective than those later employed. For instance, arithmetic is more effective than is memorizing in securing great and stable attention. But of the 43 later cases of mental application in the students all are cases of arithmetic, whereas out of the 111 earlier observations 76 are cases of memorizing.

The most striking feature of the table is the approximation of the figures in the bottom line with those in the next line but one above. On the showing of their respiration, the children either were not entertained by the devices of the experimenters or else they did not fall into genuine non-emotional revery. The first of these suppositions was rendered incredible by their behavior, and, therefore, the second has been adopted in construing the results. One should also observe the justification furnished by the figures in this table for the assumption that when the children were entertained their attention was stable. The figures for the children in the third line from the bottom should be compared with those for the students in the first line of the table. In mental application in the students attention was by hypothesis stable in the highest degree.

As regards *changes in the regularity* of breathing, the following observations may be made upon the figures: There are eight total sets of cases in the table. It is practically certain that in one of these sets, the cases of annoyance in children, attention became in general exceedingly unstable. In this set of cases, the expiratory pause became more irregular in 70% and the depth of breathing in 80%. In each of the seven other sets, it is probable that attention became more stable in the majority

of cases. In all of these sets save one, the length of the expiratory pause tends unequivocally to become more regular. Moreover, the tendency toward greater regularity in the expiratory pause is most pronounced in exactly those sets of cases in which it is most certain that attention became more stable, namely, in mental application in both classes of subjects, and in both kinds of pleasurable absorption in the children. The exception to the rule is constituted by the cases of attention to pleasant reading. In the later observations the pause tends to become more irregular, and in the total number of observations the tendency toward greater regularity is slight. The irregularity is doubtless accounted for by respiratory movements verging toward laughter. It seems best, therefore, to exclude this class of cases from consideration as regards changes in the regularity of either the expiratory pause or the amplitude. On the whole, the figures for the expiratory pause offer a rather neat confirmation of the conclusion that breathing becomes more or less regular as attention becomes more or less stable.

So much cannot be said of the figures for the amplitude. It is clear that the amplitude tends to become more irregular when attention becomes less stable than the normal. It is true also that the amplitude tends to become more regular in three out of the four sets of cases in which it is most certain that attention became more stable. It is not, however, by any means clear that under "all experimental conditions" the amplitude becomes more regular when attention becomes more stable. Witness the cases of revery and the later and more perfect observations of attention to unpleasant reading in the students and also the cases of mental application in the children. In the interpretation of these cases, we fall into a dilemma. *A priori*, it would be easy to object that perhaps the effect of mental arithmetic upon the children and of unpleasant imagery or idleness upon the students was not to secure stable attention. Yet in these three sets of cases the expiratory pause did become more regular. We must suppose, therefore, either that the increase in the regularity of the expiratory pause is meaningless or else that the depth of breathing became more irregular in despite of the fact that attention became more stable. The writer makes the second supposition.

The evidence furnished by the other groups of experiments is now in order. The results of the music experiments are of doubtful validity. Out of 155 stimulus-tracings in which modifications of regularity were carefully studied, the expiratory pause became less regular than the corresponding normal pause in 25.2% and more regular in 14.2% and the amplitude became less regular than the corresponding normal amplitude in 24.5% and more regular in 12.9%. Although the method by which

these modifications were determined was loose and inaccurate, yet it is fairly evident that the tendency was toward irregularity. The length of the pause was probably affected by variations in the loudness of the music. The writer conjectures that the breathing rhythm was also somewhat affected by the music rhythm.[1] These tracings all belong to the period in which long compositions were played on the organ. Had these same curves been correlated with the hymn-tunes, one might suppose that especially in subjects of a kinæsthetic turn of mind, the irregularity was due to singing-movements imperfectly suppressed. In the somewhat similar experiments of Binet and Courtier upon a single subject, a similar irregularity was found. These writers conjecture that it is "le développment des idées et des sentiments qui trouble la respiration, tout en tenant compte des effets spéciaux dus à la mélodie et à l'harmonie."[2]

In the whist-experiments, changes in the expiratory-pause were not studied at all. Fortunately, however, as regards the regularity of the amplitude, the experiments are more satisfactory than the music experiments, at least on the face-value of the figures. Inferences in regard to changes in regularity are very uncertain since each phase of attention was assumed to last only a very short time, sometimes only ten or twelve seconds. The tracings are divided, therefore, simply into tracings which are as regular as the normal, tracings which are slightly irregular and tracings which are very irregular. The results may be sketched as follows: With the subject G. (the writer), out of 18 tracings taken during "study," that is, reflective attention to a given hand, 66.7% are as regular as the normal; out of 15 tracings taken during "suspense" or "anxious attention," 53.3% are as regular; out of 9 tracings taking during "relief," 66.7% are as regular; on the other hand, out of 59 available tracings taken during gratification or "pleasure," 67.8% are very irregular; and out of 50 tracings taken during "chagrin," 48% are very irregular and 16% slightly irregular. With the subject H., out of 21 study-tracings, 61.9% are as regular as the normal; out of 35 available suspense-tracings, 74.3% are as regular; out of 20 relief-tracings, 45% are as regular as the normal but 35% are very irregular and 20% slightly irregular; out of 68 available pleasure-tracings, 82.4% are very irregular; and out of 78 chagrin-tracings, 74.4% are very irregular. In view of the unsatisfactory nature of the standards of normal breathing, of the fragmentary character of the tracings, and of the guess-work by which the subject's

[1] The curves have not been studied with special reference to this point.

[2] L'Année Psychologique, 1896, pp. 114-115.

states of mind were determined, the correspondence between these two sets of figures is remarkable. Only the figures for relief indicate any divergence of tendency and here the number of observations upon the first subject is very small. Certainly the tendency in emotional excitement toward greater irregularity in the depth of breathing is too heavily marked to be overlooked.

Only the tracings obtained from the spaniel P. remain to be discussed. One hundred tracings were secured as long as the full circumference of the drum. Fifteen of these curves were so broken by the movements of the dog as to be thrown out. The remaining eighty-five are divided thus: Dog greatly frightened, trembling violently and sometimes panting, with occasional yawning inspirations, 10; dog moderately frightened and trembling slightly, 27; dog slightly frightened and trembling at intervals, 22; dog quiet and drowsy, 18; dog asleep, 8. This trembling affected the spaniel's hind-quarters chiefly, and did not seem, of itself, to produce gross irregularities in the tracings although it sometimes gave a wavy appearance to the inspiration-line. Both the amplitude and the expiratory pause were studied in these tracings. The results are as follows: When the dog was in a state of repose, whether she was awake or asleep, both the amplitude and the expiratory pause were strikingly regular.[1] Even out of the 22 curves taken when the dog was slightly frightened, the amplitude is very regular in 11 and the pause in 14. On the other hand, out of the 27 tracings taken when the dog was moderately frightened the amplitude is very irregular in 18, and out of the 10 tracings taken when she was badly frightened it is very irregular in all. The irregularity produced by fear in the expiratory pause is much less marked. Out of the 27 tracings of moderate fear, it is very irregular in 13 only, whereas in 7 out of the 10 tracings of great fear it is eliminated and is regular in the sense of regularly failing to appear. These facts close the evidence in regard to regularity.

It will be remembered that the positive assertion of the Angell-Thompson thesis, which it is the purpose of this investigation to verify, is that the correlation between regular breathing and stable attention and between irregular breathing and unstable attention is essentially constant. In making this particular assertion the writers do not distinguish between regularity of amplitude and regularity of expiratory pause. The results of the experiments here reported would seem to show, first, that the regularity or irregularity of the one does

[1] This great regularity is not exhibited by the tracings taken in repose from the spaniel A.

not imply the regularity or irregularity of the other; second, that the correlation between the regularity or irregularity of the expiratory pause and the stability or instability of breathing is "essentially" constant in human beings; third, that the amplitude tends in the large majority of cases to become irregular when attention becomes unstable; fourth, that the amplitude does not tend to become regular in all groups of cases in which the testimony or behavior of the subjects and the regularity of the expiratory pause indicate stable attention.

It will be remembered that MacDougall[1] found great irregularities in the depth of breathing correlated with mental application. This irregularity he attributed to the concomitant shallowness of breathing and to "periodically increased innervation due to incipient asphyxiation." For the cases here reported this explanation is insufficient. The breathing of both students and children tends to become shallower in mental arithmetic, but the breathing of the students tends to become more regular in depth and that of the children does not. Moreover, a study of the individual tracings from the students reveals no definite correlation between shallowness and irregularity of depth. MacDougall also suggests that irregularity during mental arithmetic may be due to a fluctuation in the intensity of the effort required. This explanation doubtless holds for the irregularity in some of our arithmetic cases. One must also take into account the tendency in some of the children to disobey orders and call out the result at each step in the process. None of these explanations, however, touch the irregularity in the revery-cases. Here thoracic breathing becomes noticeably deeper, and one is reminded of the assertion of Mosso that "when one does not attend closely, the diaphragm tends to become quiet and the thorax makes larger but irregular movements."[2]

For convenience, *changes in the absolute size of the amplitude and in the absolute length of the expiratory pause* will be discussed together. In the eight sets of cases in Table 1, we have all the four possible combinations of tendencies to change in these two features of respiration. In only one of the eight sets is the level of attention assumed to fall. These are the revery-cases. Here alone breathing (1) tends to deepen at the same time that the expiratory pause tends to become shorter. In all the other seven sets of cases the level of attention is assumed to rise above the normal. In three of them, breathing (2) tends to become shallower while the expiratory pause tends to

[1] The Physical Characteristics of Attention, *Psych. Rev.*, 1896, pp. 158-180.

[2] "Die Ermüdung," Quoted by Angell and Thompson.

shorten. Of these three sets, one is the group of mental-application cases in the students. Here we have exactly that shallow and rapid type of breathing which has been found by so many investigators[1] in correlation with mental effort or "voluntary" attention. Another of the three sets is that of the unpleasant-reading cases in the students. Here also attention probably increased in stability as well as degree, so that there was a certain kind of "mental application." The third set is that of the annoyance cases in the children. This result is in flat opposition to the finding of Binet that breathing deepens in fear, disgust, and anger. We have now taken up four of the eight sets of cases in the table. In two of the remaining four groups, breathing (3) tends to become shallower, whereas the expiratory pause tends to lengthen. These are the pleasant-reading cases in the students and the arithmetic cases in the children. Finally, we have the last possible combination of effects in the last two sets of cases, those in which the children were "pleasantly entertained." Here breathing (4) tends to deepen whereas the expiratory pause tends to lengthen. This last combination of effects must almost inevitably retard the breathing,[2] and is therefore in opposition to the results of Binet who found accelerated breathing and an abbreviated expiratory pause in fairly constant correlation with a rise in the attention-level.

Before theorizing on these combinations, it will be well to take into account the results not included in the table. Out of nine pain (pinching or pricking) experiments upon the students the breathing became noticeably shallow in eight. Out of the eight experiments in which unpleasant circumstances were voluntarily recalled, the breathing became shallow in five. This is the only unequivocal testimony as to tendencies toward changes in form which is furnished by the pain and voluntary-emotion experiments. In the music-experiments, modifications in the size of the amplitude and expiratory pause were not adequately studied. The figures for the pause are not worth giving. Out of the 155 tracings discussed under the head of regularity the amplitude was greater than the normal in 29% and less in 21.9%. In the whist experiments, as already stated, the expiratory pause was not studied at all. In regard to amplitude, the results may be briefly indicated as follows: In the study-tracings of the subject G., the amplitude was less than the normal in 50% and greater in 22.2%; in the suspense-tracings, it was less in 53.3% and greater in 20%; in the relief-

[1] Mentz, Delabarre, Binet and Courtier, Lehmann, Meumann and Zoneff, MacDougall.

[2] Theoretically, the relative rapidity of muscular contraction and relaxation must also be taken into account.

tracings (only nine in number) it was less in 22.2% and greater in 22.2%; on the other hand, in the pleasure-tracings, it was greater than the normal in 60% and less only in 8.3%, and in the chagrin-tracings it was greater in 46% and less in 24%. With the subject H. the amplitude was less than the normal in 47.6% of the study-tracings and greater only in 4.8%; in the suspense-tracings, it was less than the normal in 55.5% and greater in 13.9%; in the relief-tracings, it was less in 30% and greater in 15%; in the pleasure-tracings, on the other hand, the amplitude is greater than the normal in 34.8% and less in 15.9%; and in the chagrin-tracings, it is greater in 25.6% and less in 24.4%. The two sets of figures agree in showing a tendency toward shallower breathing in mental application and toward deeper breathing in pleasurable excitement. As the breathing in pleasure was more rapid as well as deeper than the normal, the expiratory pause was almost certainly shorter. The breathing of G. also tended to be deeper and faster in chagrin. This chagrin, be it noted, was not an exhilarating form of anger but was commonly an unpleasant mixture of disappointment and mortification.

Turning to the spaniel-tracings, one finds that in repose and in slight alarm, the dog P.'s breathing was very shallow with a long expiratory pause. In moderate fear, her breathing became still shallower and the expiratory pause was still longer as well as less regular. These tracings have a peculiar "thready" appearance. On the other hand, when she was intensely frightened her breathing deepened in every case and the expiratory pause was eliminated. As noted above, when P. was extremely frightened she often panted with lolling tongue and also yawned at intervals.[1]

There are strong resemblances between the tracings taken from the dog P. in extreme fear, the pleasure and chagrin tracings of the subject G., the pleasure and relief-tracings of the subject H., and the mass of revery-tracings taken from the students. In all these groups, the amplitude tends to deepen and to become more irregular and the expiratory tends to shorten. This deep and rapid breathing is the type which Binet considered characteristic of emotion. Probably most of the tracings of the whist experiments which are labelled pleasure or chagrin are really relief-tracings as well. Attention suddenly relaxed when the subject or her partner won or lost

[1] The same symptoms of fear have been noted under other circumstances in other cockers belonging to the writer. A. exhibits them in the railway cars. It should be noted in passing that A.'s normal breathing is not "thready" like P.'s. A.'s curves in their moderate depth, their comparative irregularity and brief expiratory pause resemble the tracings of a little child.

the crucial trick of a hand or succeeded or ignominiously failed in establishing a long suit. Attention in such cases may remain at a relatively high-level, but, in spite of the affective disturbance, there is, to the best of the subject G.'s self-observation, a distinct fall of level, a release from the tension of preceding moments.

For the unification of the various data in regard to the amount of amplitude and expiratory pause, the writer hazards the following conjectures: First, a given type of breathing, deep, irregular, and with little or no expiratory pause often characterizes the lapsing of effort or rather of those nervous conditions which keep attention stable at a high level and which sometimes result in the effort-consciousness. This panting breathing occurs in profound affective disturbance in which the stability of attention is broken up, in cases of sudden relief in which there is a sharp drop in the attention-level, and in cases of revery in which the attention-level falls very low. It may well be objected that this type of breathing is not that of sleep. But revery is neither effort nor sleep but a transition-state which may have different physical characteristics from either. It is impossible, however, to blink the fact that when the children were annoyed their breathing was shallower in the majority of cases and the expiratory pause longer in a large minority of cases (46.2%).

Second, when the level of attention rises, there is a general tendency for breathing to become shallower. This tendency may be part and parcel of the muscular inefficiency found in mental effort by MacDougall, and supposed by him to indicate "a reduction in the degree of reflex stimulation throughout the organism and inferentially a greater efficiency in the central nervous discharges." An exception to this general tendency is the deep breathing which, on the showing of Binet, often characterizes emotion. Setting aside the spaniel-tracings, the only exception which appears in these results is the deepening of breathing in marked cases of pleasure when attention still remains stable. In the large majority of each of the two sets of cases in which the children were agreeably entertained, the breathing deepened although the expiratory pause tended to lengthen. Thus deepened breathing would seem to be a concomitant both of relaxation without pleasure and of pleasure without relaxation. This supposition in regard to pleasure contravenes the opinion both of Binet and of Angell and Thompson. The writer is not prepared to press it. It is perfectly true that breathing did not tend to deepen when the students were entertained by reading. The students, however, were not half so well amused as the children. At any rate, one finds a striking contrast between the deepening of the chil-

dren's breathing when they were highly gratified and its shallowness when they were sharply annoyed.

Third, when the level of attention rises, the expiratory pause is shortened except when attention is strongly expectant. In expectation, which indicates a sort of suspended adaptation of the organism, the subject "pays breathless attention."[1] This view covers both the lengthening of the pause in moderate fear and in entertainment. Alike in the pleasant reading cases in the students and the entertainment cases in the children, the subjects were waiting for a *denouement*.[2] This conjecture is suggested by the remark of one of the student experimenters[3] that when the children were frightened or amused "the centre of interest was a 'What next?' involving physical restriction, rather than a glowing or indignant appreciation of the present, tending toward acceleration." It may be added that in the nut-cracker and varnish cases the pause lengthened notably,[4] whereas, when the boys were kissed, it shortened as conspicuously. This lengthening of the pause does not seem seriously to conflict with the results of Binet since he secured only a few cases of protracted expectation. Perhaps the lengthening of the pause when the children were doing mental arithmetic is not covered by this explanation. But aside from the probability that attention was in their case strongly expectant from step to step of the "sum," the length of the expiratory pause was undoubtedly modified by the tendency to speak at certain intervals.

Before one turns away from the consideration of changes in the form of breathing, it should be mentioned that on the showing of these experiments, changes in the amplitude, changes in the length of the expiratory pause, and changes in the regularity of either amplitude or pause all seem to be independent of one another. By no turning or twisting of Table 1 can any correlation between these modifications be made out. Moreover, the writer has worked out for the individual tracings of the first group of experiments a table of coincidences between the various changes and has reached a purely negative conclusion.

Changes in the rate of breathing must be treated with the utmost brevity. Tables 2 and 3 if clearly explained will speak

[1] Binet and Courtier recognize that after a shock, breathing may for the moment be retarded. This suspension they regard as an adaptation phenomenon. (*Op. cit.*, p. 81.)

[2] Expectation was much less marked during the unpleasant reading. Attention was held by the imagery presented.

[3] Miss A. S. Carlisle.

[4] In such cases there is often a marked inspiratory pause also or a peculiar "hump" or "foot-hill" in the inspiration line.

TABLE 2.

*Changes in the Rate of Breathing with Changes in the Degree and Stability of Attention.*

| Group of Experiments. | Nature of Stimulation. | Slow Breathing. | | | | | | | Fast Breathing. | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | No. of Observations. | Increase. | | | Decrease. | | | No. of Observations. | Increase. | | | Decrease. | | |
| | | | Total Cases % | Followed after Stimulation by | | Total Cases % | Followed after Stimulation by | | | Total Cases % | Followed after Stimulation by | | Total Cases % | Followed after Stimulation by | |
| | | | | Increase Cases % | Decrease Cases % | | Increase Cases % | Decrease Cases % | | | Increase Cases % | Decrease Cases % | | Increase Cases % | Decrease Cases % |
| 1 Subjects: Students. | Mental arithmetic, Memorizing, | 56 | 80.4 | 12.5 | 66.1 | 17.9 | 12.5 | 3.6 | 98 | 74.5 | 10.2 | 62.2 | 22.4 | 6.1 | 16.3 |
| | Listening to pleasant reading. | 12 | 83.3 | —— | 66.7 | 16.7 | 8.3 | 8.3 | 27 | 48.1 | 14.8 | 33.3 | 48.1 | 29.6 | 11.1 |
| | Listening to unpleasant reading. | 19 | 84.2 | 47.4 | 36.8 | 15.8 | 5.3 | 10.5 | 33 | 51.5 | 15.2 | 33.3 | 42.4 | 18.2 | 24.2 |
| 2 Subjects: Children. | Mental arithmetic, | 28 | 82.1 | 21.4 | 50.0 | 14.3 | 7.1 | 7.1 | 21 | 42.9 | 9.5 | 33.3 | 57.1 | 33.3 | 19.0 |
| | Entertainment, | 24 | 54.2 | 20.8 | 33.3 | 37.5 | 12.5 | 20.8 | 31 | 35.5 | 12.9 | 19.4 | 61.3 | 29.0 | 32.3 |
| | Annoyance, | 23 | 56.5 | 13.0 | 43.5 | 34.8 | 17.4 | 13.0 | 25 | 40.0 | —— | 40.0 | 56.0 | 28.0 | 28.0 |

TABLE 3.

*Breathing Rates Correlated with Different Kinds of Attention.*

(*All rates are calculated per minute.*)

| Group of Experiments. | Nature of Stimulation. | Slow Breathing. | | | | Fast Breathing. | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Rate for Preliminary Tracings. | Stimulus-Tracings. | | Rate for Relief Tracings. | Rate for Preliminary Tracings. | Stimulus-Tracings. | | Rate for Relief Tracings. |
| | | | Number. | Average. | | | Number. | Average. | |
| 1 Subjects: Students. | No stimulus: Normal, | — | 240 | 14.2 | — | — | 265 | 21.2 | — |
| | No stimulus: Revery, | — | 10 | 15.0 | — | — | 35 | 22.1 | — |
| | Arithmetic, | 15.7 | 25 | 20.9 | 17.2 | 21.9 | 53 | 23.9 | 21.0 |
| | Memorizing, | 16.1 | 31 | 17.9 | 17.1 | 22.2 | 45 | 23.3 | 22.1 |
| | Pleasant reading, | 15.8 | 12 | 17.8 | 16.9 | 21.2 | 27 | 21.3 | 21.6 |
| | Unpleasant reading, | 15.3 | 19 | 17.2 | 17.7 | 20.9 | 33 | 21.0 | 19.2 |
| 2 Subjects: Children. | No stimulus: Normal, | — | 103 | 19.0 | — | — | 99 | 27.1 | — |
| | Arithmetic, | 19.2 | 28 | 21.4 | 19.5 | 26.9 | 21 | 26.6 | 24.8 |
| | Entertainment, | 18.7 | 24 | 20.7 | 19.8 | 26.2 | 31 | 26.1 | 26.1 |
| | Annoyance, | 18.9 | 23 | 22.7 | 19.1 | 27.8 | 25 | 26.6 | 22.9 |

for themselves. The detail which it is most important to explain is the division of the observations into cases of slow and cases of fast breathing. This division was made because in looking over the rates for the different conditions of attention it appeared that the mean variation would be enormous. When the division was actually made and the two sets of cases studied separately, it appeared that the rates were modified differently under stimulation. This showing, however, was the result of making the division and not the reason for doing it.

The division was made in this way: The arithmetical mean was found for all the "normal tracings," short or long, of each class of subjects. The normal tracings from the students are 505; the average rate is 17.9 inspirations per minute; the mean variation is 3.5. The normal tracings from the children are 202; the average rate is 23.1; the variation is 2.5. Each set of observations which included both a preliminary tracing and a stimlus tracing was classed as a case of fast or slow breathing according as the rate of the *preliminary tracing* did or did not exceed the average normal rate for the whole class of subjects, young women or children. In the revery experiments, each tracing is classified according as its total rate did or did not exceed the average normal.[1]

Three other details should be explained: (1) The mean variations from the averages in Table 3 have been omitted merely to save space. The ordinary variation is two and some tenths. (2) For the sake of saving space in Table 3, the number of rate-counts averaged is given only in connection with the averages of the stimulus-rates. In a few cases the relief-tracing corresponding to a stimulus tracing is lacking, and in the experiments in memorizing syllables and poetry one of the preliminary tracings averaged as such answers to two stimulus-tracings. The tracing interpolated between the two stimulus-tracings is taken account of in Table 2 but cannot properly be averaged either as a true preliminary or as a true relief-tracing.[2] (3) In Table 2, the percentages in the light-faced type pertain to the same total numbers of observations as the percentages in the heavy-faced type. To illustrate from the first line of the table, out of 56 cases of slow breathing the rate rose during arithmetic or memorizing in 45 cases or 80.4%. It rose during stimulation and continued to rise afterwards in 7 cases or 12.5% of 56; it rose to fall again afterwards in 37 cases or 66.1% of 56 and so on.

Turning to the significance of the figures in Tables 2 and 3, one may make the following cursory observations: First, slow breathing strongly tends to be accelerated as the level of attention rises. This tendency is paramount in all groups of cases although, in the entertainment and annoyance cases of the children, it is opposed by the tendency already noted for the expiratory pause to lengthen in expectant attention. Second, in

[1] The writer now sees that it would have been better to find the "central value" rather than the arithmetical mean and to draw the division-line at this point. However, the experiments really do not seem at best precise enough to justify the doing of all the intricate rate-work over again.

[2] See page 271.

rapid breathing the tendency for the expiratory pause to lengthen in expectant attention is so weighty as sometimes to overbalance any tendency toward acceleration which may accompany the rise in level. Thus both in slow and in rapid breathing, high-level attention, if expectant, produces a disturbance of type, in one case accelerating and in the other tending to retard. Third, in mental work on the part of the students, the tendency toward acceleration is marked in both types of breathing. Here attention, though, strained, was practically non-expectant. Fourth, in the students' arithmetic cases, the average amount of acceleration is much larger in both types of breathing than it is in any other set of cases from either class of subjects. (See Table 3.) Fifth, if the rate of breathing rises during stimulation, it tends to fall again afterwards, and if it falls, it tends to rise again afterwards. The exceptions to this rule, save for the few arithmetic cases, may be explained on the suppositions that the affective disturbance often persisted after the stimulus which occasioned it was supposed to have come to an end, and that in the memorizing experiments the students indulged in mental rehearsals during the relief-tracings. The fact that the rate tends to rise again afterwards when it falls during stimulation militates against the Binet and Courtier supposition that the fall after rise is a fatigue phenomenon.[1] It seems rather to be a lapsing of disturbance. Sixth, in the relaxation of revery, breathing is slightly more rapid than the normal. (See Table 3.) It may well be asked why, if this acceleration is due to the relaxation, we do not find acceleration in all the so-called "relief-tracings." The answer is that acceleration does not necessarily accompany relaxation in the sense of a return to the ordinary attention-level, but that it tends only to accompany relaxation in the positive sense of a fall below the normal level or of a sharp fall accompanied by the specific organic relaxation-consciousness.

To bring this long presentation of experimental results to an end, it remains only to add the data on rate-changes not included in the table. In the pinch-and-prick experiments in which the threat to hurt produced strong apprehension, slow breathing was considerably accelerated and rapid breathing was enormously retarded. Rapid breathing was in two cases suspended as long as the pinch lasted—upwards of ten seconds. If there were no mental expectation after a pain began, there was at least a difficult adaptation. It may be added here that the figures for the nut-cracker experiments, in which the children were frightened, would have made still lower the annoyance-average for rapid breathing in Table 3 had it not been for

[1] *Op. cit.*, pp. 52, 53.

a few cases of enormous increase in the experiments in which the boys were kissed. In one of these cases the stimulus-rate rose to 50.8 inspirations per minute. The average for rapid breathing is in the 13 nut-cracker cases, 24.8; in the 9 kissing cases it is 31.4.

In the music-tracings, the average for rapid breathing is slightly below the normal average for rapid breathing, and the average for slow breathing slightly above the normal average for slow breathing. As nothing can be said about the size of the expiratory pause in these tracings, the fall in rapid breathing cannot be explained. Here ends the evidence from massed results. As a whole, the rate-data tally exactly with the finding of Delabarre that persons who naturally breathe slowly show a much greater tendency toward acceleration when their attention is engaged than do persons who breathe rapidly.[1]

Only the evidence from the whist and dog experiments now remains to be added. Both subjects in the whist experiments breathe very rapidly under normal conditions and breathed throughout these experiments faster than their normal rate. G. breathed most rapidly in relief (24.7 inspirations per minute) and next rapidly in chagrin. H. breathed most rapidly in the cases labelled suspense (26.3 inspirations per minute), a fact which, so far as it goes, cuts straight across the expectation-theory advanced above. In these cases, however, her attention was probably at its very highest level.

For the dog P. the rate-averages are as follows: Great fear, 17.5 inspirations per minute; moderate fear, 14.3; slight fear, 13.9; waking repose, 10.9; sleep, 11. This acceleration in fear can scarcely be held to conflict with the expectation-theory since such fear in a dog, like the most overwhelming fear in human beings, is not definitely expectant. As a matter of fact, in moderate fear P.'s expiratory pause lengthened and the acceleration is due to the shallowness of her breathing. The mean variations from these averages for P. are not larger than the variations from the averages given in Table 3. It is probable, however, that the pressure of the belt tended to retard the dog's breathing. When A. is wearing the belt, her breathing rate varies from 10 to 40 inspirations per minute. Yet the average natural breathing rate of A. and of another young, but full-grown cocker now owned by the writer, is about 22 inspirations per minute when sitting awake and alert upon the lap, and about 18 or 19 inspirations when asleep.

It will be remembered that the negative assertion of the Angell-Thompson thesis is that the correlation between more and less regular breathing and more or less stable attention is

[1] Revue Philosophique, Vol. XXXIII, pp. 639 ff.

the *only* essentially constant correlation between respiration and consciousness. Such as they are, the results of this investigation tend to show that this formulation, like its historical predecessors in the same field, is too simple. The acceleration of slow breathing when the level of attention rises is essentially constant, the acceleration and deepening of breathing in relaxation is essentially constant, and the lengthening of the expiratory pause in expectation is significantly constant.

In conclusion, the points at which the results fail to fall into line with those of Binet and his co-workers may be indicated. Binet recognizes two types of breathing as variations from the normal which are produced by mental activity; first, the rapid, shallow breathing of intellectual work, and second, the rapid, ample breathing of emotion. This investigation reveals many instances of these types. In conflict, however, with the thesis of Binet it presents many instances of shallow breathing in emotion, of continuously retarded breathing in mental activity, and of rapid and ample breathing in relaxation.

On the whole, the writer is left in doubt as to whether the Angell-Thompson thesis or the Binet thesis, both alike too simple, can better be fitted into the facts. A suspicion also remains that the significance for respiration of pleasantness and unpleasantness may recently have been under-rated.

# AUDITORY SENSATION IN AN ELEMENTARY LABORATORY COURSE.

By PROFESSOR MAX MEYER, University of Missouri.

The following description of the manner in which I let my students perform certain experiments in an elementary laboratory course is offered to those of my colleagues who, like myself, might have found the corresponding recommendations of Titchener in his *Experimental Psychology* unsatisfactory, without, perhaps, seeing a way of improving upon them.

Whoever has attempted to teach an average student the most elementary facts of auditory sensation will readily concur in Titchener's statement that 'Sensations of tone are, perhaps, of all sensations, those which the average student approaches with the greatest diffidence and the least interest.' The problem before the instructor is, then, not merely *what* to teach, but even more *how* to teach it so that the diffidence of the student will be overcome and his interest awakened. Now, it seems to me that we cannot with certainty make a student confident and interested in some kind of work unless we insist from the start on his complete success in whatever experiment we tell him to perform. The student who merely pretends to have observed what he is expected to observe, is sure to lack confidence and interest in the next experiment he is told to take up. To insist upon success, however, is easier in theory than in practice. It is possible only if two conditions are fulfilled: 1. The problem set must not require a power of concentrating one's attention beyond the capacity of the student at the time. 2. The instructor must be able to convince himself, by other means than the student's mere assertion, that the student has actually observed the phenomenon in question. Let us now apply these conditions to the particular case.

Titchener devotes, rightly, I think, six pages exclusively to difference tones. Difference tones are certainly as interesting in themselves and as important for the theory of hearing as the phenomena of color contrast, *e. g.*, are in another field of sensation. When we read, however, the directions which Titchener gives to the student and the instructor, we must be struck by the fact that the above two conditions are not fulfilled.

1. The student is given two Quincke tubes and told to observe that, when sounded together, they produce an additional

tone which is a double octave lower than the lower primary tone, or, in another case, a single octave lower, or like the tone of a certain given third tube. Now, according to my experience with students, this is too difficult a task for the average student. He is expected to make up his mind whether he does or does not 'hear a tone' while the only characterization of that state of consciousness which his instructor calls 'hearing this tone' is the name 'lower double octave of the lower primary tone.' This means something to a musical student, a student who enters the laboratory with a considerable ability to concentrate his attention upon auditory phenomena. To the average—even piano-playing—student it amounts to practically the same as being told to think in a language which he never learned.

2. The instructor has no means of determining whether the student 'heard the tone' except the affirmative answer of the student. But this is at least as unreliable as a student's answer to the instructor's question, at the end of a lecture, whether the student understood the lecture. How reliable the answer is in this latter case, every teacher knows well enough. Therefore, instead of asking the above question, he will use other means of finding out what the student has learned. Should we not, in a laboratory course, too, use other means of finding out whether the student really observed what he is expected to observe?

Having had very good success with my methods of introducing the average student into observing difference tones, I wish to describe these methods here, in the hope that others, who might imitate them, will be equally successful with them.

I give to the student two whistles, either Galton whistles or Quincke tubes. The latter being particularly cheap, it might be best to speak of their use here, although Galton whistles are much easier to handle. I give to the student two Quincke tubes of rather high vibration frequency (not less than 2,000, the higher, the better), attached to a single mouth piece of glass by means of a short tin tube closed with cork stoppers of which one carries the mouth piece, the other the Quincke tubes. I show the student that it is easy to suppress either of the tones by covering the opening of the resonance tube with a small piece of writing paper. I further call the student's attention to the fact that he can make the tone of a sounding tube *lower* by narrowing the free end of the resonance tube by means of a small piece of wax. (It is best to put on a piece somewhat too large and to melt out gradually by means of a large, somewhat heated nail as much as is desired.) I now let the student listen successively to one and the other tone, calling his attention to the fact that both are very high and not very different.

I must say here, for the reader, that the difference of vibration rate may be about 200 or 300. I now let him sound both tubes simultaneously and call his attention to the rather low, humming tone, a tone so different from the primary tones that a student hardly ever fails to notice it immediately. This, however, is not the end of the first experiment, but only its beginning, merely a preliminary step. I now set before the student a definite task which it is within his power to perform. I give him a felt hammer and a tuning fork the vibration rate of which differs by 50 or 100 vibrations from the difference tone. I tell him that the difference tone is lower (or higher, as the case may be) than the tone of the tuning fork, but that it can be made equal to it by making the whistle tones more (or less) equal to each other. And then I leave him alone with the task of thus *tuning* the difference tone in unison with the fork tone. It is extraordinarily rare that a student does not succeed in this, although some take more time for it (perhaps one or even two hours) than others.

The reader will easily comprehend the advantages of the method just described. The difference tone is so different from the primary tones that even a student with a minimum ability to concentrate his attention upon tones notices it without difficulty. And then, what is even more important, the task of tuning this very difference tone compels him to concentrate his attention on the tone again and again for an hour or so. This is of the utmost importance because it trains him in sound analysis. It is extremely difficult for the instructor to train a student's ability to analyze a sound by simply telling him to try, try, try again and again, to convince the student that this is an effective way of acquiring this ability if he does not possess it. The student, as a rule, will not do it. Here, however, the mechanical task of tuning the whistle induces him again and again to do what the instructor wants him to do, to pay attention to a partial phenomenon, the difference tone produced, and induces him much more effectively than the instructor's mere advice. Still more important, however, is the fact that the instructor can easily tell if the student actually succeeded in observing the difference tone. He certainly could not by accident have *tuned* a tone correctly which he did not *distinctly hear*.

To give the student further training in what he needs so much, in the ability to direct his attention to partial sounds, I give him a bass bow and two tuning forks on resonance boxes, say of 450 and 525 vibrations, and instruct him in applying the bow to the *side of one* of the tines half an inch below the end. (I never use for this purpose the top surface of a tine, because bowing at the top surface is more difficult and tears the hair

out of the bow; besides, there is usually some tuning wax on the top surfaces of my forks, which would be ruinous to the bow.) I further give him a fork without resonance box, but with adjustable weights, ranging from about 60 to 90 vibrations, and tell him that the difference tone of the other two forks is identical with one of the tones of this fork. I then place the weights at one extreme and set before the student the task, not of merely listening to something, but of *doing* something, of tuning the adjustable fork in unison with the invariable difference tone. At the same time I use the opportunity of introducing him into another phenomenon by telling him that he will clearly hear beats when the tones approach each other, and that by making these beats slower and slower until they disappear he can obtain the greatest accuracy of tuning. When the student has succeeded, he calls the instructor who easily convinces himself as to whether the difference tone and its beats have been observed. With regard to the beats he can do this by slightly mistuning the fork and requesting the student to tap with his finger in the rhythm of the beats.

When the student has performed these two experiments he has acquired to a considerable degree what is an absolutely necessary condition for any work in auditory sensation, the ability voluntarily to direct his attention to a partial sound, an ability which so few students possess when they enter the psychological laboratory. I now feel justified in making him undertake work in which he must be practically without any aid or supervision on the part of the instructor. I give him a series of forks on resonance boxes representing the numbers from 1 to 12, say the forks 100, 200, 300, etc., up to 1,200, or the forks 75, 150, 225, etc., and tell him to verify for himself the most important laws concerning difference tones. These laws I give him in a preceding lecture, and I copy them below as I have previously published them. (*Zeitschrift*, 16, p. 2.)

It is essential for these experiments that the student be in possession of sources of sound which produce pure tones and can be tuned in definite intervals with the utmost accuracy, since otherwise the observations would be too difficult at this stage of the student's training. Therefore I should not use for these experiments sources of sound other than good tuning forks on resonance boxes. It is further advantageous now to make two students work together. In the preceding experiments one could do the work as well alone. But here, since it is well to bow the forks in quick succession in order to obtain two equally strong tones, and since it is usually necessary for the observer to find a place where the difference tone is strongest, sometimes near the resonance boxes, sometimes in a particular position to the walls of the room,—the bowing of the forks should be done

by an E student and the O student be left free to concentrate his full attention on the observation.

The laws which the student is to verify for himself, do *not* express *all* the difference tones which one might possibly hear in every possible combination of objective tones, but merely those difference tones which one is *most likely* to hear in those combinations which correspond to relatively simple ratios of the vibration rates and are therefore (musically and otherwise) *particularly interesting*. These laws are the following four:

1. In case the ratio of the vibration rates does not differ much from 1:1, say in 11:12, or in 9911:9989, a single difference tone is audible, whose pitch corresponds to the pitch of a tuning fork the vibration rate of which is equal to the difference of the vibration rates of our case. In addition to the difference tone, however, beats are usually clearly audible, and a 'mean tone' is audible too, which lies between the two primary tones. If the interval is quite small, this mean tone is usually more pronounced than either of the primary tones, particularly when we hear with one ear only, having the other ear plugged. The beats just mentioned seem to be the fluctuations of the intensity of the mean tone rather than of the primary tones, if we use one ear only.

2. A second class of ratios which is of particular interest, is that of the ratios whose numbers differ by 1. In each of these cases the difference tone 1 is audible, but often quite a number of additional difference tones can be perceived. If the numbers of the ratio are rather small, as in the case—5:4, all the tones from 5 down to 1 are without any great difficulty noticeable. As we study ratios of increasing numbers, the tones following directly upon 1 (in a rising direction, of course) seem to have a tendency to drop out. And if we go on in the same way, we soon find only one difference tone left, the tone 1. We have then simply reached a case in which the difference tone is determined by the first law above. The accompanying table represents this class of ratios with their difference tones.

| Obj. Tones | Diff. Tones easily audible |
|---|---|
| 2, 1 | – |
| 3, 2 | 1. |
| 4, 3 | 2, 1. |
| 5, 4 | 3, 2, 1. |
| 6, 5 | 4, 3, ?, 1. |
| 7, 6 | 5, 4, ?, 1. |
| 8, 7 | 6, 5, ?, 1. |
| 9, 8 | 7, 6, 5, ?, 1. |
| 10, 9 | ?, 1. |

3. A third class of ratios to be studied are the ratios made

up of comparatively small numbers, representing intervals less than an octave. In these cases three difference tones are often easily noticeable, one corresponding to the direct difference of the vibration rates (u-l); one corresponding to the difference between the latter number (u-l) and the vibration rate (l) of the lower primary tone, *i. e.*, (2l-u); and one corresponding to the difference between the just mentioned differences (u-l) and (2l-u), *i. e.*, (2u-3l). It is to be noticed, however, that a difference tone is hardly ever audible which corresponds to a difference *larger* than the subtrahend; for example, the primary tones 9 and 5 produce the difference tones 4 and 1, but not 3=4-1, or at least not an easily noticeable tone 3, 3 being larger than 1. The following table contains a few examples of this class:

| Obj. Tones | Difference Tones |
|---|---|
| 8, 5 | 3, 2, 1. |
| 5, 3 | 2, 1. |
| 9, 5 | 4, 1. |
| 7, 4 | 3, 1. |
| 11, 7 | 4, 3, 1. |

4. The fourth class to be studied are the ratios made up of comparatively small numbers, representing intervals larger than an octave. The first fact to be noticed here is the lack of a difference tone corresponding to the direct difference of the two vibration rates. Such a tone, if audible, would lie between the primary tones. As a rule, only one difference tone is easily noticeable in these cases, which can be found according to the following rule: Find the *smallest* difference between the larger number of the ratio and any multiple of the smaller number. The table contains a few instances of this class:

| Obj. Tones | Difference Tone |
|---|---|
| 11, 4 | 1=3x4—11. |
| 12, 5 | 2=12—2x5. |
| 9, 4 | 1=9—2x4. |
| 11, 3 | 1=4x3—11. |
| 5, 2 | 1=5—2x2. |
| 8, 3 | 1=3x3—8. |

When the student has faithfully done this work, he has a fairly good idea of the laws of tone perception so far as difference tones are concerned, and a considerable ability to control his attention in auditory observation. But I wish to give him further training. To accomplish this without having to fear a decrease of interest on the student's part, I give him a practical problem the solution of which does not merely require him to listen, but again to *do* something. I tell him that to be

familiar with difference tones is not only of theoretical, but also of practical importance, that one may apply such a knowledge to the problem of tuning an organ according to a prearranged plan. I now place him before my experimental organ (a reed organ with two manuals, the tones of the upper manual differing from those of the lower, described in *Zeitschrift*, 33, p. 292) and tell him to determine by means of difference tones the exact vibration rate for each key. The data which I give him are: 1. the fact that one of the *f*'s has the vibration rate 1024 (it does not really have this vibration rate, but it makes the computation easier, without, of course, changing the ratios), and 2. the fact that when the vibration rate for any key has been found, those of its octaves are determined by either dividing or multiplying by 2. I further advise him to use exclusively the lowest difference tone audible, and never to use the difference tone of a combination representing an interval larger than a Fourth, because in larger intervals the lowest difference tone does not correspond to the direct difference of the vibration rates. Now and then helping him a little, I make him find out all the vibration rates and *ratios* on my organ. For example, he combines $f=1024$ with the next higher *a* and finds by pressing down other keys and comparing that the difference tone is the *f* two octaves below. He knows then that the unknown *a* is equal to 1024 plus one-fourth of 1024, that is 1280. Or, having found *b* equal to 1440, he combines it with the following *c* and finds that the difference tone is four octaves below this *c*. He then finds the vibration rate of *c* from the equation

$$c=1440+\left|\begin{matrix} c \\ 19. \end{matrix}\right.$$

Thus I give the student, not only further training in sound analysis (the average student cannot have too much of it!), but also an introduction into the musical significance of the different ratios. Titchener introduces the student into this matter on pp. 32 and 33 by means of two tables, a picture of a key-board, and the necessary explanations in words. However excellent this brief introduction of Titchener may be, I doubt if any student derives much benefit from it. The student is certainly far more familiar with the relations existing between vibration rates and their ratios on the one hand, and the keys of a key-board on the other, if he has found these ratios by his own labor, working on the instrument itself and solving a practical problem the possibility of the solution of which with no other help than his own ear never entered his mind.

Only now should I recommend to have the student work on overtones. These are far more difficult to observe than difference tones. But after such a considerable training in sound analysis as described above the student is able to convince him-

self without too much difficulty of the existence of overtones and their significance for what the organ builder (and I with him, in opposition to Titchener[1]) would call 'quality' of a tone.

Concerning 'summation tones' I do not tell my students anything in an elementary laboratory course, since I do not regard them as being of any direct importance to the psychologist.[2]

There are only two further classes of experiments on auditory sensation which I prescribe to my students: 1. Experiments on noise. 2. Experiments on the briefest stimulus producing a tone. The experiments on noise are done by us in about the same manner as recommended by Titchener, except that I do not tell my students that tones and noises are two fundamentally distinct classes. The experiments on the briefest stimulus are performed in this way. I give the student an 18 inch disk (of zinc or aluminum) with 150 holes, a rotation apparatus, and a support with rubber tube and two end pieces of glass for blowing, further a tuning fork of, say, 600 vibrations and a felt hammer. O has to blow and observe, E to increase very gradually and constantly the speed of rotation and to observe to the extent he can. O now and then sounds the tuning fork very briefly and compares its tone with the tone of the siren disk. When the tone of the latter has become equal to the siren tone, he stops the experiment. I have devised this form of the experiment in order to be sure that a 'tone' is heard produced by the siren. If it were not a 'tone,' it could not be said to have risen to unison with the fork. Of course, in the above case no one would doubt the existence of a real tone sensation; but one might doubt it in the following modifications of the experiment. The student glues a paper ring over the holes, leaving only 6 or 7 consecutive ones open. The experiment is repeated. Instead of a continuous tone, a series of tones of very short duration is observed. In every other respect the experiment is the same as above. Now a further hole is closed by a piece of paper. The observation is more difficult, but the result is the same, a tone is audible. Another hole is closed, and so on, until only two holes are open. The student observes that the tone decreases in intensity while the number of holes approaches 2, since in all our sense organs the sensation does not possess its full intensity at the very time when the stimulus begins to act, but has to rise to it. This natural rise is impossible when the stimulus is extremely shortened. This decrease in intensity together with the noise accompanying each blow of a hole is responsible for the increasing

[1] See my article on 'The Attributes of the Sensations,' *Psychol. Review*, 11, 1904.

[2] See my article 'Ueber Kombinations- und Asymmetrietöne,' *Annalen der Physik* (Vierte Folge) 12, 1903.

difficulty of observation. However, the student is able to observe thus a tone even when only two holes are open, because he begins the experiment with an easy case and is given a chance to develop his power of observation as the difficulty of observation increases. Finally, the student leaves only one hole open, in which case an increase of the speed of rotation does not lead to any similarity between the siren sound and the fork tone which might be called unison. *I. e.*, the ends of the auditory nerve fibres need not receive more than two shocks to give rise to a sensation of tone. (The noise we hear is the result of irregular reflections of each single puff.)

## SUMMARY.

The following experiments are recommended in the order in which they are enumerated and in the manner described above:

1. Tune a difference tone to unison with a fork.
2. Tune a fork to unison with a difference tone. Observe the beats.
3. Observe the difference tones of as great as possible a number of combinations of two objective tones each.
4. Apply your knowledge of the difference tones to the problem of tuning an organ.
5. Observe overtones and 'quality of tone.'
6. What is a noise? Produce noises by divers means and explain them.
7. How many shocks of the nerve ends are necessary to produce a sensation of tone? (Siren experiment.)

# AUDITORY TESTS.[1]

BENJAMIN RICHARD ANDREWS.

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## CHAPTER IV.

### TESTS OF MUSICAL CAPACITY.

The ability to appreciate music cannot be measured simply and directly, since musical perception is a conscious experience of wide range and very great complexity. It remains to approach the problem indirectly, through the examination of the more fundamental factors which contribute to the apprehension and enjoyment of musical form. Music involves a sequence of single notes (melody), or more usually a sequence of tonal complexes (harmony), possessing in either case rhythmical and metrical characteristics, and evoking an affective or emotional

[1] Continued from Vol. XV, January, 1904, p. 14.

response on the part of the hearer. In musical perception, we may accordingly distinguish four factors, an examination of which will afford a view, even if but a partial view, of the musical capacity of an individual:

1. The discrimination of single notes heard in succession.
2. The discrimination of groups of simultaneous notes (chords) heard in succession.
3. The perception of rhythm.
4. The emotional or affective response to music.

Our musical tests fall then, naturally, into four groups: pitch discrimination, chord discrimination, rhythm, and affective response. To these is added a fifth group of miscellaneous tests and methods. The paper is intended to present a systematic view of the whole field; not a programme necessarily to be followed in its entirety in individual cases of testing.

I. *Discrimination of Tones.* The problem, here, is to measure the individual's capacity for discriminating successive tones of different pitch. The test will consist in submitting to the observer a single tone of a certain pitch, followed, after a brief interval, by a second tone of different pitch, and in requiring judgment as to the pitch relations of the two tones. This problem has been approached experimentally by Gilbert[1] and by Seashore,[2] the former employing an adjustable pitch pipe, the latter, a series of tuning forks. Their methods will be set forth presently.

Several preliminary matters must first be disposed of. (1) In any judgment of pitch, a difference in intensity may be mistaken for a difference in pitch. This is to be avoided by keeping the intensity of the successive sounds as nearly as possible uniform.[3] (2) Stern has shown that certainty of judgment is affected by the length of the time interval between the two tones.[4] He names six seconds as optimal, and though a shorter period, 2-3 seconds, may be used, it should at any rate be kept uniform. (3) More important is the form of judgment required. The subject may be asked to state each time whether the second tone is "like" the first, or "different;" or whether the second tone is of the same pitch as the first, or is higher or lower than it. Gilbert used the former and Seashore the latter form of judgment. It is recognized that at the limen it is much easier to judge whether two tones are alike or different in pitch,

[1] J. A. Gilbert: *Experiments on the Musical Sensitiveness of School Children, Studies from Yale Psych. Lab.*, I, 80.

[2] C. E. Seashore: *Hearing Ability and Discriminative Sensibili for Pitch, Univ. of Iowa Studies in Psych.*, ii, 55.

[3] C. Stumpf, *Tonpsychologie*, 1883, I, 315, mentions a case in which judgments of pitch-difference were always made in terms of intensity. The louder tone was always judged to be higher.

[4] L. W. Stern: *Zeitsch. f. Psych. u. Phys.*, xxi, 1899, 377-379.

than to state particularly whether the one tone is lower or higher in pitch than the other. In the one case, the judgment is of simple difference; in the other, of qualitative difference of direction.[1]

The judgment of qualitative difference is to be preferred, since (a) it tests directly the sort of sensible discrimination required in singing or playing, or in listening to music—in such cases one is interested in the rising and falling of notes, and not in the existence of simple difference; and (b) it is more difficult, and hence gives a more rigid test than discrimination of simple difference.[2]

We may now consider various tests of pitch discrimination.

A. *Determination with Tone-tester.* The instrument employed is an adjustable pitch-pipe, or "tone-tester," which is fully described by Gilbert,[3] who used it in his tests. It has a range over the octave from $f^1$ to $f,^2$ and is scaled to test pitch discrimination at $a^1$ of 435 vibrations (in some pipes, 450 vibrations). The tester is mounted at the base of a fan-shaped board and an index arm continues its adjusting lever to a scale on which are subdivisions for thirty-seconds of a tone, over a half-tone above and below $a^1$. Uniform intensity of sound can be approximately secured by using the residual air in the lungs after normal expiration. The instrument is especially suitable for mass-tests, as in a schoolroom, and in such use judgments may be written.

The method to be employed with the tone-tester is that of minimal changes, 'with knowledge.' The following series of pairs of tones is submitted: a′ and a′, a′ and a′ + 1/32, a′ and a′ + 2/32, . . . . . and this is continued to some pitch of a′ + n/32 such that it lies well above the point at which the second tone is always judged to be of "higher" pitch than the first.

[1] W. Preyer: *Grenzen d. Tonwahrnehmung*, 1876, pp. 2, 36; Stumpf: *op. cit.*, I, 313.

[2] The writer computed Seashore's and Gilbert's results to see if differences could be found which might be ascribed to the use of the two different forms of judgment. Seashore's work on adults (p. 56), excluding six who could not recognize a difference of a semi-tone, gives an average discrimination of .142 of a tone for 18 women, and of .173 of a tone for 25 men (all college students). Gilbert, working with children from 12 to 19 years old, found an average discrimination of .112 of a tone. This seems to indicate the influence of the two sorts of judgment used, as the limen falls lower in Gilbert's work, which was based on the easier judgment of simple difference. The age difference in the two cases does not affect this conclusion, since the limen decreases with age (Gilbert), and here the younger subjects have the lower limen. The different instruments employed may, in some measure, account for the discrepancy in the limens obtained in the two investigations; but doubtless the different form of judgment is the real explanation.

[3] Gilbert: *op. cit.*, 81.

The "ready" signal is given two seconds before each pair to ensure maximal attention. The series is then reversed in order, beginning at this upper value $a' + n/32$, and a descending series given: $a'$ and $a' + n/32$, $a'$ and $a' + n - 1/32$, etc.[1]

The subject should be informed which way the changes are to progress. The pitch in the ascending series which is first judged "higher," and the pitch in the descending series which is last judged "higher" before passing to the judgment of "equal," are averaged for the first result. Five or ten tests should be made and the results averaged.[2] The mean variation should be computed as a check on errors.

B. *Method with tuning forks.* This method, which employs a standard fork of $a'$ (435 vibrations) and a series of forks differing from it by from 1 to 30 vibrations, has been used by Seashore.[3] The series of forks includes, in addition to the primary $a'$ fork, nine secondary forks whose vibration rates are respectively 1, 2, 3, 5, 8, 12, 17, 23 and 30 vibrations higher than 435. At this point on the scale, a difference of 54 vibrations is equivalent to a difference of a whole tone. Accordingly, the tone of the primary fork followed by that of the fork one vibration higher gives a pitch difference of 1/54 of a tone; and with the other forks it gives pitch differences of 2/54, 3/54, 5/54, etc., of a tone. The forks should be set in vibration by striking them against a rubber cushion, or the palm of the hand, and with care to secure as nearly as possible uniform intensity. The forks should always be held in the same position before the ear; the best position is just before the meatus, with the plane of the two forks parallel to the side of the head. The standard tone is sounded first, and judgment required as to whether the second tone is the same as, or higher than, the first. The method is like that with the tone-tester—the average is taken of an ascending and descending series of minimal

[1] Gilbert uses, as his second series, a descending series below $a'$: thus, $a'$ and $a'$, $a'$ and $a' - 1/32$, $a'$ and $a' - 2/32$, . . . . . He then averages the results of the ascending series above $a'$ and the descending series below $a'$. This involves determination of the limen at two distinct points on the scale, and the averaging of these unrelated values. The method recommended above obviates this by finding values in both ascending and descending series for the same liminal band on the scale. In tests of this degree of fineness, the two methods would scarcely give different results. The distinction between them, however, should be kept clear, and it is advisable to keep to the more accurate procedure.

[2] Gilbert found an average discrimination of a little more than 5/32 of a tone. He correlates discrimination with age and finds a fairly regular increase from .384 of a tone at 6 years to .075 of a tone at 19 years.

[3] Seashore: *op. cit.*, 59, and *Educ. Rev.* xxii, 1901, 75.

changes with knowledge,[1] and then the determination is repeated five or ten times.[2]

C. *Other tests of Pitch Discrimination.* The Tests Committee[3] of the American Psychological Association makes some suggestions regarding tests of perception of pitch.[4] Cattell and Jastrow recommend the adjustment of "one monochord or pipe to another, the tones not to be sounded simultaneously;" and Jastrow suggests the use of two Gilbert tone-testers for this purpose. Baldwin and Sanford recommend the selection of "a match from a set of forks, giving a fixed number of vibrations per second more or less than a standard, *e. g.*, a standard fork of 500 vibrations per second, and other forks 497, 497.5, 498, etc., 500, 500.5, 501, 501.5, etc." The Committee suggests no plan of procedure. The intention is apparently to put the manipulation of instruments in the hands of the persons tested. This procedure would open the way to variable errors from differences in the intensity of the tones used, the position of the forks before the ear, etc. In any case, a single determination would furnish no measure of pitch discrimination. Two tones are subjectively equal if the second falls anywhere within the limits of indiscriminability of difference above and below the first tone. A single adjustment to subjective equality might accordingly by chance hit upon objective equality, or might exist with an objective difference in pitch of any amount up to the limen. Tests which involve

[1] Seashore's method is somewhat different, and it may be followed instead of the one suggested. Starting with the widest interval (made by the forks $a'$ and $a' + 30$ vib. sounded successively) he gives one trial at each step until the region of uncertainty is reached; then ten trials at each step. He takes as the limen the lowest interval for which there are eight correct judgments in the ten tests made. The method recommended has an advantage over this of Seashore, in that the averaging of results from ascending and descending series provides against the error of habituated judgment.

[2] On the basis of tests on 110 adults and 380 children, Seashore suggests tentatively the following scale for the interpretation of results: (1) a child whose limen is 2/54 of a tone or less may become a musician; (2) 3/54-8/54 should have a plain musical education, and singing in school should be obligatory; (3) 9/54-17/54, singing in school optional, and a plain musical education should be given if special inclination for music is shown; (4) 18/54 or above, should have nothing to do with music (*Educ. Rev.*, xxii, 76). This scale is not only arbitrary, but its application would result in injustice. It is not fair to draw final conclusions as to musical capacity from tests on a single factor, as pitch discrimination. Moreover, pitch discrimination depends in good part on practice: a person who could just discriminate a difference of 20/54 of a tone might, with practice, reduce this to 10/54. For such a case, see Seashore's own results, *Univ. of Iowa Studies*, II, 64.

[3] *Cf.* preceding portion of article, this *Journal*, Jan., 1904, 23.

[4] *Psych. Rev.*, iv, 1897, 134.

"matching" or "adjusting" to equality, can, therefore, only be carried out by the method of average error; *i. e.*, they require a series of adjustments, 10 or more, and are measured by the average of the absolute deviations (regardless of + or —) from objective equality. Still the errors due to the subject's handling the instruments persist. Accordingly the two methods outlined are, perhaps, to be preferred to those recommended by the Committee.

It seems especially desirable to suggest some method which can be used for rough and rapid work, in case the instruments recommended are not at hand. It is quite possible with the piano or organ to make tests which will disclose the presence of extremely poor pitch discrimination, though, of course, no measurement of the degree of discrimination is possible. The person to be tested should be placed so that he cannot see the key-board and care should be taken to strike the keys with equal force, and to allow an equal interval between the members of each pair of notes. Such a series as the following should be submitted, the "ready" signal being given before each pair of tones, and the tones presented in succession: c′—c′; c′—e′♭; g′—g′♯; g′—g′; g′—f′♯; d′—d′♯; d′—d′; e′—f′; c′—c′♯; g′—g′♯; g′—g′. The person is to judge whether the second tone is the same as, or higher or lower than, the first. In this way, it can be determined in a few moments whether a person always recognizes correctly pitch differences of a semi-tone or more. The tests may be repeated in the octave above and below the middle octave of the piano, to determine whether differences in pitch are equally well perceived with high and low tones. Such a test admits of simultaneous application to a large number of individuals, as in a schoolroom. Rough tests may also be made for intervals less than a semi-tone by using an unmounted adjustable pitch pipe, like that adapted for use in the tone-tester. By setting its adjusting arm for small differences, especially where the interval on the adjusting scale is large, it is possible to sound successive tones varying by 1/2 or 1/3 a semi-tone, *i. e.*, 1/4 or 1/6 a tone. In this way, with the piano and the adjustable pitch pipe, a rough examination of capacity in pitch discrimination can be made.

II. *Discrimination of Chords.* Another factor involved in musical capacity is the perception of simultaneous tonal combinations. Such combinations, as ordinarily employed in music, are composed of three or more simultaneous notes and are called chords; combinations of two simultaneous notes form harmonic intervals. The latter lie at the basis of the former, and, for convenience' sake we may test them rather than the more complex combinations. Tests of two kinds are suggested:

(A) The perception of identity or difference in harmonic intervals given successively.

(B) Judgment as to the simplicity or complexity of single harmonic intervals given without knowledge of their objective tonal composition.

A. *The recognition of successive harmonic intervals.* This test measures the recognition of likeness and difference in successively perceived harmonic intervals. The person is asked

to pass judgment of "like" or "different" on pairs of intervals successively given on a piano or organ. A series like the following may be used: 5th—5th; 5th—octave; major 3rd—minor 3rd; major 3rd—major 6th; minor 3rd—minor 6th; major 2nd—minor 2nd; 5th—major 3rd; 5th—minor 6th; 5th—5th; 5th—minor 3rd; major 7th—minor 7th; octave—octave; octave—major 7th; etc. The two intervals compared must always have a common note. The results may be expressed in percentages, and the individuals' results referred to the average result.

B. *Unitariness or Plurality in Harmonic Intervals.* This test involves judgment as to unitariness or plurality, or as to the degree of unitariness, in harmonic intervals submitted without knowledge of their composition. Stumpf includes this as one of his four tests to determine unmusical ears.[1] This test takes into account the fact that the constituents of an interval are heard out with a facility which varies according to the interval involved, and which further varies directly with the musical ability of the person tested. The latter fact has given rise to two methods which we will state, though the first alone is practicable for inexpert use, as in tests. The average untrained observer, under certain conditions of experimentation, mistakes the octave for a single tone in about 75% of his judgments; the 5th, 40-60%; the 4th, 28-36%; 3rd's and 6th's, 20-30%; 2nd's and 7th's, about 15%[2]. This may be expressed by saying that the octave is the closest fusion, and that the other intervals are less closely fused, in the order given.

(a) The first test of perception of degree of fusion is used in psychological determinations for untrained, unmusical subjects, and is the only one which can ordinarily be used in tests. It consists of submitting various harmonic intervals, one by one, to the person tested, and requiring a judgment whether one note or two notes are heard. In most instances it will scarcely be possible to go through a sufficiently long series to warrant comparative percentage results for all the intervals. It is suggested that 30 or more intervals be submitted for judgment, including octaves ($c^1$—$c^2$), 5th's ($c^1$—$g^1$), 3rd's ($c^1$—$e^1$ and $c^1$—$e^{1\flat}$), and 2nd's ($c^1$—$d^1$ and $c^1$—$d^{1\flat}$). These intervals should be arranged in an irregular order and single notes interpolated among them as a check on judgments given; thus: $c^1$, $c^1$—$g^1$, $c^1$—$c^2$, $g^1$, $c^1$—$d^1$, $c^1$—$e^1$, $e^1$, $c^1$—$c^2$. . . . At each step the subject

[1] C. Stumpf: *Tonpsych.*, ii, 157. The other tests are: singing a note which has been played on the piano; judgment as to which of two successive tones is the higher; and judgment as to the more pleasant of two successively given fusions.

[2] C. Stumpf: *ibid.*, 135, 140, 142 ff.; the conditions are stated in E. B. Titchener's *Experimental Psychology*, Vol. I, Pt. I, 330 ff.

will state whether he hears a single note or two notes. It will thus be possible to determine how the person perceives the intervals of high, medium, and low degree of fusion. One may expect mistakes to occur frequently, perhaps more than half the time, in judgments of the octave; less frequently with the 5th's and 3rd's; and only occasionally with the 2nd's. If results vary considerably from the values given above, more extended tests may be made. Individual results may be roughly referred to the percentages given, or the average results found.

(b) In case the person tested is musical, another method, the direct method of paired comparison, may be used. In this method, the various intervals are compared, each with every other, and comparative judgment passed directly as to which is the better fusion. In such a judgment, the pleasantness or unpleasantness of the impressions, and considerations of musical significance, and of nearness of the constituent tones in the scale, must be abstracted from. The certainty with which one interval is judged a better fusion than another should be noted, as this is an important indication of degree of unitariness. The results as before may be compared with the percentage scale given above, or each result compared with the average. This method is, however, little suited for unskillful use.

In both methods the piano or organ will usually be found most convenient. The first method can be readily adapted to school or other mass tests.

By these two tests, first the recognition of likeness or difference in successive harmonic intervals, and secondly, the perception of unitariness of harmonic intervals, reliable indications of musical capacity may be obtained, so far as this capacity depends upon correct perception and discrimination of tonal complexes.

III. *Tests of the Perception of Musical Rhythm.* As noted in the introduction, music is not simply a composition of simultaneous and successive tones, but it possesses rhythmical and metrical characteristics. A musical composition, as a progress in time, is divided into equal temporal units called measures, and into larger divisions, composed of groups of measures, called "phrases," "sentences," "periods," etc. The measure is indicated in printed music as the group of notes falling between two perpendicular lines or bars on the staff. The conception of the measure involves two factors: the durations of the pitches which "fill out" the measure; and the stresses or accents which the notes receive. In the first place, the temporal sum of the notes of successive measures is the same. In the second place, the pattern or scheme of accents or stresses remains the same in successive measures—a heavy stress on the initial note

of the measure and one or more weak stresses on the following notes in the measure. A measure extends from one heavy stress to the next. There are two fundamental rhythmical patterns: that of two accents to the measure, the first strong, the second weak, hence known as 2-part or duple rhythm, march rhythm and common time; and that of three accents to the measure, the first strong, the last two weak, hence known as triple rhythm or waltz time. There are possible many accentual patterns, but all are reducible to these two fundamental forms. The same pattern usually continues throughout the same composition. A musical composition is thus a succession of groups of notes, or measures, each conforming to the accentual pattern of the piece. The equal duration and the accentual similarity of successive measures, are the primary reason for music appearing as a temporal movement with related parts, that is, as a rhythm.

In addition to the rhythm dependent upon similarity between measures, there is a larger rhythm in music based upon the stressing of the larger structural units, the "section," "phrase," "sentence," and "period." These terms are used variously, but they indicate groups of measures, composed of two, four, eight, or more measures, as the case may be. The smaller groups are component parts of the larger groups: two of the 2-measure groups follow each other and make a 4-measure group; two of the 4-measure groups make an 8-measure group, etc. The initial part of a large group is stressed as is the initial note of a measure, and major and minor accents, respectively, set off the larger group of measures from one another, and indicate their subdivisions or the smaller measure-groups within them. There are thus accentual patterns having reference to the phrase, sentence, etc., just as the 2-part or 3-part pattern has reference to the measure. So there is a larger rhythm in terms of these groups of measures.

The tests[1] suggested refer first to the rhythm directly depend-

[1] There is some experimental evidence of individual variations in the sense of rhythm. Sears, *Ped. Sem.*, viii, 1901, 19, secured information by a questionary of several hundred children who showed difficulty in keeping time in the movements of marching, dancing, and calisthenics. He refers to statements of teachers, army officers and dancing masters, that there are wide variations in the ability of individuals to appreciate the rhythm of music and respond to it with proper movements. Some cases reported were deficient in rhythmical sense and remained so despite training; others, who had been deficient, had improved under training. In his experimental work, however, Sears found only 9 out of 1,297 children who were entirely unable to repeat by tapping objective rhythmical series set them. Bolton (*Amer. Jour. of Psych.*, vi, 1894, 185 and 204) found a larger percentage, 2 out of 50 persons examined, who were quite deficient in rhythm. His method was to submit a series of uniform metronome clicks to his subjects

ent upon similarly accented measures; second, to the larger rhythm of the phrase, sentence, etc. Perception of rhythm of the first sort can best be examined with non-musical sounds, and there are suggested: (A) Tests for *involuntarily* hearing a succession of non-musical sounds as a rhythm series; (B) Tests for hearing such a succession as a rhythm, *voluntarily or with suggestion*. To this section are added certain tests for hearing, as a rhythm, a succession of musical notes, in which variations in duration, quality and intensity occur; and, also, a test for recognizing the fundamental two-part and three-part accentual patterns of musical measures. (C) Tests are suggested for the larger rhythm referred to, the perception of musical phrases and sentences.

A. *Tests of Involuntary Rhythmisation.* The test used by Bolton, referred to above, is to be employed here. It consists in submitting to the subject a series of successive sounds which are entirely identical and follow each other at a regular brief interval; the subject is to report how he hears them, *i. e.*, whether as entirely similar, disparate sounds, or whether he subjectively arranges them into accented rhythmical groups.

The details of such a test are as follows: A metronome[1] is used which gives clicks of the same intensity and quality, and is adjustable to various rates, from 40 to 208 per minute. The bottom should be removed from the base of the metronome and it should be set on a pad of heavy cloth or felt, to deaden the sounds. One of the more rapid rates, 152 or 200 beats per minute, should be used, as this is more favorable for subjective rhythmisation, and it should be continued for 45 seconds. The following general directions should be given the person tested: he is to put himself in a receptive and passive attitude toward the sounds; he is not to fix his attention closely on each sound as such, but simply listen to the successive beats and tell how he hears them, whether he notices anything peculiar about them. Any reference to rhythm, or to the fact that this is a test of rhythm must be avoided. The first series should be repeated three times; and then other repeated series should be given, in which the 200, 152 and 92 rates at least should be included.

With some persons, rhythmical groupings of the uniform sounds will take place almost unconsciously, and manifest themselves in some way; perhaps, in an involuntary counting, "one—two, one—two," or in other accented verbal accompaniment; perhaps, in movements in time of hand, head or foot; or by some accompanying imagery, auditory or visual, as the ticking of a clock, a mental picture of a person walking, the

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and only two failed to group them in some rhythmical order. It is important to note that these two persons "possessed no appreciation of music at all, they could not carry a tune." Yet they must have had some notion of music for "they were able to recognize some of the common airs when they were sung or whistled."

[1] Titchener: *op. cit.*, Vol. I, Pt. 1, 174, and Pt. 2, 338. The tests A and B under Rhythm follow Titchener quite closely.

blows of a hammer, etc. In some such way these persons will, involuntarily, manifest their subjective transfer of the series of uniform sounds into a succession of similar accented groups. If they are at all keen, introspectively, they will at the same time report that every second, third or fourth sound, as the case may be, is stressed, and forms the initial member of a group of two, three or four sounds, and that the interval between groups is longer than that between the individual sounds. Other persons will hear only the succession of similar sounds, each like the preceding, and each separated from the others by a uniform time interval; and until some more or less direct suggestion of rhythmisation is made to them, they will not transpose the uniform series into rhythmical groups. The former class possess good rhythmical sense; the latter are more or less deficient. Both classes are to be examined further by the tests with suggestion, described in the next section.

B. *Rhythmisation with Suggestion.* The first test under this head is to determine the individual's normal rhythmical grouping, *i. e.*, the grouping into which sounds most easily fall when he comes to group them rhythmically. If the preceding tests show many instances of subjective rhythmisation, the person's normal rate will already be evident. Even in such cases, however, it will be well to go through these additional tests in which some indirect suggestion is made to bring out subjective grouping more prominently. Suggestion may be effected as follows: The subject may be asked to count the beats of the metronome, and asked later how he counts them; or if he said the sounds were "like a clock," he may be asked if they differed in quality and intensity as the ticks of a clock do, and in this way his attention may be directed toward grouping the sounds (Bolton). In case these suggestions fail to evoke groupings, the subject should be asked outright to note if the beats do not group themselves, as he listens to them. By this means, the person may be led to make subjective groupings. The results will show what grouping (usually the two-group) is favored.

The second division of this test measures the person's ability to throw a uniform series of sounds at will into any grouping desired. The series already given should be repeated, and others given at slower rates, while, in each series, the person suggests to himself arrangements of the sounds into groups of 2, 3, 4, 5, 6, 7, or 8 sounds each. The 2-group, and multiples of it up to 4, 6, and sometimes 8, will usually be found easiest to suggest; the 3-group, fairly easy; the 5-group, usually difficult; and the 7-group, extremely difficult. Imagery will be found useful in assisting suggestion: a pendulum for 2-groups; a triangle, perhaps, for 3-groups; a square for 4-groups, etc.

The first part of this experiment examines the individual's capacity for involuntary rhythmical grouping, and this, with the test on rhythmisation under suggestion, will throw light on the rhythmical sense of the person tested. Those who, in the first test, give no evidence of subjective rhythmisation and in the latter test are not able by any sort of suggestion to effect a grouping of the sounds, may be considered deficient in a sense of rhythm.

With the above tests several supplementary tests of the sense of rhythm may be mentioned. In each, the series of uniform sounds is replaced by a series in which piano notes are used instead of metronome clicks, and in which a periodic variation in duration, time interval, or quality is introduced.[1]

(a) Varied duration. The following series of notes is played with equal intensity upon the same piano-key: quarter—half—quarter,quarter—half—quarter,etc.; quarter—quarter—quarter—half, quarter—quarter—quarter—half, etc.; quarter—half—quarter, quarter, etc. The longer sound ordinarily appears more intensive than the rest, and the interval following is apparently longer than the other intervals, thus giving rhythmical grouping, with the lengthened note forming the first member of the groups.

(b) Varied time interval. Series with periodically recurring lengthened intervals are played on one digital: quarter—quarter—quarter—rest, quarter—quarter—quarter—rest, etc. The sound followed by the rest appears louder than the others and begins rhythmical groups.

(c) Varied quality. Two, three or more notes *e. g.*, $c^1$—$e^1$ or $c^1$—$d^1$—$d^1$—$e^1$, are repeated over and over in a series, with equal intensity and at equal intervals, thus: $c^1$—$e^1$—$c^1$—$e^1$—$c^1$—$e^1$, etc., and $c^1$—$d^1$—$e^1$—$c^1$—$d^1$—$e^1$—$c^1$—$d^1$—$e^1$, etc. Similarly for a group of four notes. Such a series will usually be arranged in 2-groups, 3-groups, or 4-groups, each composed of the repeated notes.

As already stated, musical compositions are ordinarily written on the basis either of the 2-beat or of the 3-beat rhythmical grouping. A test is suggested of playing a duple time composition, and another in triple time. The terms "waltz," "two-step," "march," etc., carry a rhythmical suggestion, and should not be used in connection with the test. Simply submit two musical "compositions," and ask for a judgment as to the rhythmical grouping of each. This last experiment can be performed with the piano or some other musical instrument, or with a phonograph, gramophone, or even a good music box.

[1] Titchener: *op. cit.*, Vol. I, pt. 2, 348-9.

C. *The Perception of Musical Phrases and Sentences.* If a person fully appreciates music, a composition becomes for him an organized movement in which larger parts, musical sentences, follow each other in rhythmical recurrence, and, at the same time, present within themselves smaller recurring parts, musical phrases. The "phrase" usually includes 4-8 measures, and the "sentence" 8-16 measures, and a smaller grouping, of two measures, is recognized. The perception of this form or structure of music is perhaps the largest factor in æsthetical musical perception, and yet it can be tested only inadequately.

The test for phrasing will consist in presenting one or more simple and rather distinctly phrased compositions, and requiring a judgment as to the number of large divisions of which it is composed. It will be necessary to use music with which the persons are unfamiliar. Old and unusual accompaniments might be selected. The music of a foreign national anthem, if unfamiliar, might be used. It may be found necessary to preface the test with a statement of the nature of musical phrases and sentences, and quite possibly with illustrations from actual music. The piano, organ, or phonograph can be used in the tests.

IV. *Affective Reaction to Music.* The keen enjoyment which some persons secure from music, and the comparative indifference with which it is regarded by others, are evidence of individual differences in the affective reaction to music. Here, then, is a factor in musical capacity which can be examined apart from the factors already considered.

A test of capacity for musical enjoyment can best be made by using simple tonal combinations rather than complex musical compositions, as the degree of pleasure derived from the latter is so largely influenced by training, familiarity with the particular composition and with music in general, that it would be difficult to interpret the results. The "Star Spangled Banner," for example, would appeal to the average American in a degree beyond its musical merits, while the "Marseillaise" would be devoid of associations. Accordingly, simple tests with intervals and chords should first be used, though they may properly be supplemented by tests with whole musical compositions. The simpler tests are further advisable, since we have clear evidence, based on our musical inheritance, as to the normal relative pleasantness and unpleasantness of the different intervals and chords; thus, in such tests, a standard is already provided by which the individual's result can be evaluated.

The following tests are suggested:

*A.* Harmonic intervals, *i. e.*, combinations of two simultaneous tones, should be submitted for paired comparison, and

judgment asked as to which is the more pleasant or unpleasant.[1] Judgment should be passed on the following intervals: octave and 5th; octave and major 3rd; major 2nd and major 3rd; 7th and major 3rd; 5th and major 3rd; octave and major 6th; 5th and major 6th; major 3rd and major 6th; octave and minor 3rd; octave and minor 6th; 7th and major 6th; 7th and major 3rd. The results should show a preference for the 3rds and 6ths.

After these tests on harmonic intervals, the same combinations may be repeated as melodic intervals, and judgment again passed as to pleasantness and unpleasantness. The first test in the series just suggested would then be given as follows: The tones in the octave $c^1$—$c^2$ should be given in close succession, followed after 2 or 3 seconds by the two tones of the 5th ($c^1$ and $g^1$); and judgment asked as to which combination is more pleasant. Similarly with the octave and major 3rd, etc. In this series, also, judgments will probably favor the 3rds and 6ths.[2] If but one of the tests is to be given, the first, *i. e.*, the harmonic, is to be preferred.

*B.* The larger simultaneous combinations of tones—chords—should also be compared as to their relative pleasantness. The method, as before, is that of paired comparison.

Material for tests can be secured by arranging consonant and dissonant triad chords in pairs for judgment.[3] The following list is suggested for testing, and after each pair of chords, the chord is indicated which should normally be considered more pleasant. The test can be performed on a piano or organ.

| | | |
|---|---|---|
| $c^1 - e^1 - g^1$; | $c^1 - e^1 - f^1$ | 1st preferred. |
| $c^1 - d^1 - g^1$; | $c^1 - e^1 - g^1$ | 2nd " |
| $c^1 - e^{1\flat} - g^1$; | $c^1 - a^1 - g^1$ | 1st " |
| $c^1 - e^{1\flat} - f^1$; | $c^1 - f^1 - a^1$ | 2nd " |
| $c^1 - e^{1\flat} - b^{1\flat}$; | $c^1 - f^1 - a^1$ | 2nd " |
| $c^1 - e^1 - a^1$; | $c^1 - e^{1\flat} - a^1$ | 1st " |
| $c^1 - e^1 - a^{1\flat}$; | $c^1 - e^{1\flat} - a^{2\flat}$ | 2nd " |
| $c^1 - g^1 - a^1$; | $c^1 - f^1 - a^1$ | 2nd " |
| $c^1 - e^1 - g^1$; | $c^1 - b^{1\flat} - a^1$ | 1st " |
| $c^1 - a^{1\flat} - b^{1\flat}$; | $c^1 - e^{1\flat} - g^1$ | 2nd " |
| $c^1 - e^1 - g^1$; | $c^1 - e^1 - g^{1\#}$ | 1st " |

Each pair contains one consonant chord and one dissonant chord, and the term preferred is in each case given as the consonant combination. Consonance in chords in general implies pleasantness. Persons tested should be given no information regarding the chords; but should be asked simply to report each time the chord which is the more pleasant.

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[1] Stumpf, as already noted, used this as one test for unmusical ears, and found that, with unmusical persons, atrocious combinations of tones, as long as directly neighboring tones were not included, were usually not considered unpleasant. *Tonpsych.*, II, 158.

[2] Helmholtz: *Sensations of Tone*, 1895, 370: "The most attractive of the intervals, melodically and harmonically, are clearly the 3rds and 6ths."

[3] Helmholtz, *ibid.*, 212, gives a table of consonant and dissonant chords.

*C.* A final test for affective appreciation of music may be made by presenting in succession different musical compositions, and asking for preferences. So far as the persons are conscious of reasons for preference, these should also be stated. This test should at least show the presence or absence of bare liking for one piece of music more than for another. Comparative judgment may be passed on such pieces as:

1. The Marseillaise and Die Wacht am Rhein.
2. Swannee River and some modern popular negro music.
3. A waltz and a two-step, which are, as nearly as possible, equally familiar, and of equal merit as musical compositions.
4. Phrases selected from the work of different composers, or from different music by the same composer.
5. Two whole masterpieces of musical composition. If possible these should be actually performed in the presence of the persons tested, care being taken to avoid suggestions as to the identity or merits of the composition, or other remarks which would influence the judgment given. In the absence of suitable instruments, the phonograph will be a second-rate substitute.

In all these affective tests, it is important that the subject record not only whether the effect is pleasant or unpleasant, but, roughly at least, the strength of the reaction, as very pleasant, slightly pleasant, indifferent, slightly unpleasant, very unpleasant.

By these three tests, an examination of an individual's affective reaction to music is possible, first as to whether such a reaction is present, and secondly as to its strength. The first two tests will show whether the elements of musical composition, the intervals and chords, are heard with varying degrees of pleasantness and unpleasantness, and whether the preferences for certain combinations agree with the standards of affective value ordinarily received. The third test will disclose something of the nature and strength of the affective reaction to whole musical compositions.

V. *Miscellaneous Tests and Methods.* A small percentage of persons possess an absolute memory for pitch.[1] They are able on hearing a tone, to assign it its proper place in the scale, as $g^1$, $c^1$, g. To test absolute-pitch memory, the persons should be placed so that they cannot see the key-board of the piano, and then required to name single piano notes which are sounded in irregular order.

[1] F. C. French (*Psych. Rev.* IX, 1902, 40, *Mental Imagery of Students*) reports that 14 out of 117 students (12%) declared that they had an absolute memory for pitch. This percentage is unusually high, and it seems likely that, with actual experimentation, it would have been considerably decreased.

Vocal repetition of a tone given by the piano, or previously sung, tests at once the perception for tones and the ability to reproduce them vocally, and is not to be regarded as a test simply of accurate auditory perception of tone. There is a possible source of error, too, in the evaluation of the reproduced tone by the examiner. Nevertheless, this test will be found useful. Stumpf, it will be remembered, includes it in his list.

Another method, often used as an exercise in musical instruction, and capable of adaptation as a test, is to require the observer to sing notes as they are pointed out on a scale arranged conveniently on a chart or blackboard. The notes may be called for in regular succession, up and down the scale, or with larger intervals, *e. g.*, *do*, *me*, *sol*, etc. This is primarily a test of correct vocalization, and audition is concerned only so far as accurate hearing is always implied in accurate singing.

Monroe used three musical tests on small children, 2 to 6 years old, which may find application elsewhere under certain circumstances: (1) Ability to learn the scale; (2) Ability to remember the scale after two weeks; (3) Ability to learn kindergarten rote-songs and reproduce them after two weeks.[1]

Considerable information regarding musical capacity can be obtained from a series of questions put to the persons tested. Parents of children should also be questioned. The following questions are suggested:[2]

1. Musical education and training: training in public schools? private lessons, instrumental and vocal, when begun and how long continued? musical environment in childhood? attendance at operas, concerts and recitals? interest in theory of music? interest in musical composers and history of music? do parents or other members of the family show special musical ability or interest?

2. Enjoyment of music: in performance and in listening? music lessons in school? favorite instrument, voice, composer and composition? pleasure in marching to band music? pleasure in dancing? is part of enjoyment based on knowledge of music?

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[1] W. S. Monroe: *Ped. Sem.*, X, 1903, 144. His results are given in percentages. Of the boys, 34%, and of the girls, 58%, learned the scale. After a fortnight, with three intervening reviews, 29% of the boys and 40% of the girls could reproduce it. Kindergarten songs were remembered after a fortnight by 50% of the boys and 63% of the girls. On the basis of their individual reaction to the tests, and information furnished by parents, 27% of the boys and 59% of the girls are credited with special taste for music.

[2] The author is indebted for suggestions in certain questions to S. E. Sharp, *Individual Psychology*, *Amer. Jour. of Psych.*, X, 1899, 359 and 369.

3. Skill in music: vocal and instrumental? taking part in chorus singing, as in church? solo singing? playing in public? ability to carry an air? to reproduce it in one's head? by whistling? by singing?

In summarizing, it may well be reiterated that appreciation of music as an æsthetic experience is more than the factors which have been mentioned. Nevertheless, on the results of the tests and of the questions suggested, an approximate measure of the individual's "musical capacity" may be safely based. The capacity of the person to discriminate differences in pitch and in chords, to appreciate rhythm and to experience an affective reaction from music, can all be ascertained by such methods as we have described. The tests outlined will serve to disclose persons of unusual musical gifts, and to call attention to the desirability of training them; and, as well, to indicate those who are tone-deaf or otherwise incapacitated by nature from responding to musical training. A final word of warning might be said, that no child be considered hopelessly unmusical until given an opportunity for musical training. In musical capacity, much depends on practice.

## CHAPTER V.

### Diagnostic Tests of Hearing.

This section considers those tests of audition which may be employed in the diagnosis of auditory disease and defect. We pass here from the domain of psychology and mental anthropometry to that of pathology; or, more precisely, to a particular field of pathology, otology. Some of the tests already treated have otological value; others belong more exclusively to this field. Such tests are of interest from a psychological standpoint, and should be treated, even if but briefly, to make our systematic presentation complete. The tests we shall consider are the following:

(1) Diagnostic Speech Tests.
(2) Integrity of the Tonal Scale.
(3) Determination of Deafness.
(4) Pitch Difference of the Two Ears.
(5) Special Diagnostic Tests:
    (a) Weber, (b) Schwabach, (c) Gellé, (d) Rinne.

In general, it may be said that the following empirical facts lie at the basis of methods of diagnostic examination: The nature of auditory perception with diseased and defective organs varies (1) with different forms of stimulation, as with the tuning fork and the human voice; (2) at different parts of the tonal scale, as for high and low notes; and (3) with differ-

ent kinds of conduction (aerial or osseous). One or another of these facts appears in the tests to which we now pass.

I. *Diagnostic Speech Tests.* The use of speech tests in otological diagnosis depends upon the establishment of correlations between deafness or diminished activity for particular elements of speech on the one hand, and particular derangements of the auditory organ, on the other. American and English otologists have apparently done little in this field, and we can simply indicate the work done by German otologists, and call attention to the need for further study of the problem. Oscar Wolf seems to have made the first systematic application of speech to aural diagnosis. He tested auditory organs, which were variously diseased, as to the comparative ease of perception for the three classes of consonants, low, medium, and high, referred to in our chapter on speech tests. His material included these speech elements pronounced separately, and combined in words. As the result of his investigations, he correlated defective hearing for certain speech-sounds with various pathological conditions of the ear. Some of these correlations are as follows: poor perception of s-sounds is found with obstruction in the sound-conducting apparatus; loss of deep, lingual r with defects of the ear drum; and loss of the f-sound, with labyrinthine disease.[1] An important general principal stated by Wolf is that deep tones are heard with difficulty when there is disturbance of the sound-conducting apparatus. Bezold confirms this and states that, with number-word tests, loss of perception for 100 (hundert) is particularly characteristic of disturbance in the sound-conducting apparatus. Bezold gives other correlations, among them these: Poor perception of 5 (fünf) with inflammation of the middle-ear, 7 (sieben) and words with high consonants, with labyrinthine diseases. Bezold does not test perception for all speech sounds, but only for those low and high sounds which he has found most often defectively perceived.[2] Such diagnostic evidence is never entirely conclusive; it simply supplements other methods of diagnosis, and as a preliminary test gives valuable hints for further procedure. German otologists are by no means entirely agreed as to the significance of loss of perception for the various speech elements; but the facts already mentioned indicate the possibility of establishing a tentative differential diagnosis upon the results of speech-tests. American and English otologists might soon build up a similar system for English words by testing patients with definite words like the number series, and correlating defective hearing with the pathological conditions present.

[1] Wolf, quoted by Politzer, *Ohrenheilkunde*, 116.
[2] Bezold: *Funktionelle Prüfung*, 207 ff.

II. *Integrity of the Tonal Scale.* Some aural diseases manifest themselves by loss of certain tonal sensations: *e. g.*, low tones, as Wolf observed, are poorly heard if the sound-conducting apparatus is disturbed, and in certain other diseases the perception of high tones may be affected. Moreover, cases are not infrequent in which there is loss of hearing for parts of the scale, *i. e.*, tonal gaps are present; or, retention of hearing for isolated parts, tonal islands, above and below which there is deafness for tones. Tests to determine the condition of tonal perception at various points along the scale are therefore necessary. Politzer recommends for such a test the use of at least three forks: c (128 vib.), $c^2$ (512 vib.), and $c^4$ (2048 vib.).[1] Bezold has devised apparatus consisting of eleven forks with adjustable weights, two high adjustable organ pipes, and the Galton whistle.[2] This comprises his "continuous tonal series" with which it is possible to give tonal stimuli at every semitone interval throughout the scale, from a lower limit of eleven vibrations in the second to the upper limit of audibility for tones.

In interpreting the results of such tests, it is commonly stated that, with air conduction, impaired perception of high tones indicates disease of the internal ear, and of low tones, disturbance of the middle ear; but this is only conditionally valid. The loss of perception for high notes can be taken as only partially conclusive, and must be confirmed by other diagnostic evidence.[3] The loss of perception for low tones, however, is quite trustworthy evidence of middle-ear disturbance.[4] In any event, the results of both tests, and especially the test with low tones, are extremely valuable in diagnosis.

Bezold has made an application of the continuous tonal-series apparatus in tests of deaf mutes, who frequently retain audition for certain pitches and lose it for others, so that tonal gaps and islands are present. The accurate knowledge of what pitches are still audible may, as he suggests, be turned to account in the education of these defectives. The results promise, as well, to throw light on the diagnosis of functional

[1] Politzer: *Ohrenheilkunde*, III.

[2] The apparatus is made by Edelmann, of Munich, and described by Bezold, *op. cit.*, 123 ff., 217 ff., and 229 ff.

[3] Politzer: *op. cit.*, 112.

[4] Politzer notes some exceptions to it (*op. cit.*, 112), but Bezold accepts it as final (*op. cit.*, pp. 219, 222); and Lucae says that if low tones are heard normally, no essential disturbance to the sound-conducting apparatus can be present (*Arch. f. Ohrenheilk.*, XV, 280). Bezold has accordingly concluded that the peculiar physiological function of the chain of ossicles is the transmission of vibrations of low rates, corresponding to the deeper tones (*op. cit.*, 221).

auditory disturbances, and they have an important bearing on the acceptance or rejection of various theories of hearing.[1]

In addition to these qualitative tests, which examine the condition of perception at various points along the whole tonal scale, many otologists use a temporal test of similar comprehensiveness which measures the ringing-off time for forks with air conduction. This temporal test is applied at intervals of one octave or less up and down the scale. Such tests require considerable time and are only approximately accurate, since it is impossible to secure a constant intensity in successive tests, and difficult for the patient to tell just when a fork is no longer audible; yet they furnish indispensable assistance in diagnosis, as Bezold says; particularly in the manifold forms of cochlear disturbance.[2]

III. *Determination of Deafness.* Deafness may be of two forms: (1) a deafness for speech, or relative deafness, in which there may persist a very considerable ability to hear tones, and (2) absolute deafness, in which neither speech nor tones can be heard.[3]

If the deafness, whether relative or absolute, exists in both ears, its determination presents little difficulty. Each ear is examined in turn, the other, meantime, being stopped. Speech tests are used to determine relative deafness; and individual forks, or better those of the tonal series, to determine absolute deafness. The former test is practically the acuity test with speech; the latter aims to discover what tonal qualities, if any, are still sensed.

One-sided deafness presents more difficulties. If the case is relative deafness, results must be checked by Dennert's method, which, it will be recalled, consists of closing both ears and repeating the test words heard, to be sure that they were originally perceived by the ear under examination and not by the normal ear. If the case is absolute one-sided deafness, the tonal series apparatus is used: for the lower tones up to $c^1$, Bezold says that it is not necessary even to close the normal ear to prevents its functioning; in the next octave, $c^1$ to $c^2$, it is only necessary to close the normal ear with the finger to secure complete isolation. Above $c^2$, it is impossible to secure isolation even with closure, as the high notes are very penetrating. Perception for these high notes, however, can be tested indirectly. Bezold determined the ringing-off times for the various

[1] Bezold (*op. cit.*, 229-240, also printed in *Zeits. f. Psych. u. Phys.*, XIII, 1896, 161) gives results of examinations of deaf-mutes and their bearing on the Helmholtz theory of audition. *Cf.* also *Funkt. Prüf.*, 224.

[2] Bezold: *op. cit.*, 223.

[3] Bezold: *op. cit.*, 224 ff.

high forks with persons from whom the cochlea had been removed on one side, and who were accordingly entirely deaf on that side. The fork was sounded on the deaf side, and ringing-off times taken. Thus conduction-norms were secured, to which the results of high-fork tests with one-sided absolute deafness can be referred.

IV. *Pitch Difference Between the Two Ears.* If the same tonal stimulus is presented successively to the right and the left ear, it is normally perceived as of slightly higher pitch by the one ear than by the other. The difference is ordinarily small, from a few vibrations to one-fourth of a tone, and the right ear is usually the higher. This difference is a normal phenomenon, and the determination of its magnitude is a classical problem in experimental psychology.[1] The method may be briefly stated: Two forks, of the same objective pitch, give slightly different pitches, when heard, one at the right ear, the other at the left ear. With a bit of wax, the subjectively higher fork is flatted to subjective equality when heard as before. The difference in vibration rates is then measured in beats over a resonance bottle, and is transferred into an expression of the pitch difference of the two ears.

Aside from this normal pitch difference between the two ears, there are pathological cases (*diplacusis binauralis*) in which a difference of one-half or a whole tone, or even a musical third or more, appears. The method given for determining the small normal pitch difference would have to be superseded in such cases by the use of a series of forks giving differences of from one-fourth of a tone up. Another pathological phenomenon involving pitch differences in audition is double hearing (*paracusis duplicata*); in this disturbance, two distinct auditory perceptions arise from a single stimulus. It may be confined to speech or to tones, or may extend to both; but it is most common with tones. These disturbances usually arise from catarrh and inflammation of the middle ear; though many cases are explained by reference to the basilar membrane. Both demand the attention of the aurist.[2]

V. *Special Diagnostic Tests.* Otological diagnosis employs a series of special tests, based, in general, upon the alterations due to disease in the normal relation between bone-conduction and air-conduction of sound waves. For the normal ear, a fork sounds longer before the meatus with air-conduction than it does with osseous conduction—as when set upon the mastoid process of the skull. With certain diseases of the ear, this re-

[1] Titchener: *op. cit.*, Vol. I, Pt. I, 38.

[2] For both these phenomena, see Politzer, *op. cit.*, 557-558; Gruber: *Diseases of the Ear*, 137-140; Stumpf: *Tonpsych.*, I, 266 ff., 273-8.

lation is altered.[1] We can do but little more than state the tests that are based upon this principle, and explain their general significance. The actual interpretation of the results given by the tests involves so many interdependent factors and the recognition of so many exceptions, that it would be beside our purpose to make a systematic statement of the rules of auditory diagnosis. That belongs to the science of otology. The tests we shall describe are Weber's, Rinne's, Gellé's and Schwabach's.

A. *Weber's Test.* E. H. Weber discovered that if a vibrating tuning fork is placed upon the median line of the skull, while one ear is closed, as by the finger, the sound is localized in the closed ear.[2] If there is an obstruction in the middle ear, the sound of the fork is similarly localized in the obstructed ear. This localization of sound in the ear which is stopped is explained as due (1) to increased resonance of the outer aural passages (Weber); (2) to transfer of vibrations from the skull bone to the air of the meatus where they are reflected by the obstruction upon the tympanic membrane; and (3) to the altered tension of this membrane and the ossicles (Politzer). The lateralization with middle ear diseases is explained by Bezold as due to increased rigidity of the ossicles and their ligaments, which favors the transfer of vibrations from the bones of the skull to the ossicles. This rigidity interferes, at the same time, with the delicate equipoise of the sound-conducting apparatus and hinders the usual passage of sound vibrations by way of air-conduction and the tympanic membrane. The method ordinarily employed consists, as stated, in placing a fork on the median line of the skull and observing whether the sound is localized in one ear. The general interpretation of results is as follows: With but one ear affected, the localization of sound in it points almost unmistakably to an obstruction in the sound-conducting apparatus; in disturbance affecting both ears, but unequally, localization in the more seriously affected ear points to the same conclusion. On the contrary, localization of sound in the normal or less affected ear is of slight significance unless supported by other diagnostic evidences. Bezold recommends the use of the unweighted $a^1$-fork for this and the other tests with bone-conduction.

B. *Rinne's Test.* This test compares directly the difference in ringing-off time for air and bone conduction.[3] The fork is set with the base on the mastoid process and allowed to ring-

[1] See Bezold, *op. cit.*, 44 ff., for a statement of theories of bone and air-conduction.

[2] *De Pulsu, Auditu et Tactu*, 1834.

[3] *Beiträge zur Phys. d. menschl. Ohres, Prager, Vierteljahrsschrift*, i, 1855, 71; ii, 45, 155.

off; it is then transferred immediately to a position before the ear, where, if the ear be normal, it again becomes audible and continues so for some time. In such a case, the test is said to fall out positively (Rinne +, the normal result); and the result is expressed by the temporal excess of air-conduction over bone. If the contrary condition is true, and bone-conduction is better than air, the test is performed in the reverse order, first with air and then with bone-conduction, and the temporal excess is measured as before. In this case, the test falls out "negatively" (Rinne—, defective). The normal result with the $a^1$-fork is positive 30 seconds, *i. e.*, air-conduction lasts 30 seconds longer than bone (under Bezold's conditions, of course). The normal excess becomes less in old age.[1] With both ears affected by disease, and the acuity of the two ears not widely different, negative Rinne indicates a disturbance of the sound-conducting apparatus. With one ear diseased, the negative result may occur, though the conducting apparatus be intact. A normal, or nearly normal, positive Rinne, when hearing acuity is considerably reduced, taken with other diagnostic indications, excludes any affection of the conducting apparatus.[2]

C. *Gellé's Test.*[3] A vibrating fork is placed on the skull while the pressure of air against the tympanic membrane is increased by means of an air-ball fastened to a tube, or Siegle's speculum. In a normal ear, the tone is decreased in intensity coincident with increased pressure against the tympanic membrane; this is explained as due (1) to increased pressure in the cochlea by reason of forcing the ossicles inward, and (2) to changed conditions in the sound-conducting apparatus. If there is an obstruction in the conduction-apparatus, there is no decrease in the intensity of the tone heard; on the contrary, if the conduction-apparatus is intact, and the cochlea affected, there is the decrease in intensity noted in normal ears. This is Gellé's general rule; but in actual use, as with all these tests, many complicating factors must be taken into account. The test has practical difficulties, too, in the quick dying-off of the tone, and the unreliable statements of patients regarding changes in its intensity. It is chiefly employed in cases of severe disturbances of hearing, where other methods do not give definite results. (Politzer, p. 126.)

D. *Schwabach's Test.* This test compares directly bone-conduction for the diseased ear with that of a normal ear. A

[1] Bone-conduction does not decrease absolutely in old age, but runs parallel with the decrease in acuity of audition. Bezold: *op. cit.*, 166.

[2] Reference must be had to Bezold, *op. cit.*, 55-122, especially p. 116, and to Politzer, *op. cit.*, 124-5, for complete statements of the interpretation to be given results.

[3] *Precis des maladies de l'oreille*, Paris, 1885, 339-40.

sounding fork is placed on the mastoid process of the ear to be examined, and when it is no longer heard, it is transferred to the mastoid process of the examiner's normal ear, and the excess time noted; or, if necessary, the procedure is reversed, and the excess for the diseased ear measured. In the first case, the diseased ear has sub-normal bone-conduction; in the latter, super-normal. The general principle for interpreting results is that disease of the external or middle ear is accompanied with super-normal (prolonged) bone-conduction; and disease of the internal ear, with sub-normal (shortened) bone-conduction. The test is not valid by itself. Super-normal Schwabach with negative Rinne, however, indicates an obstruction in the sound-conducting apparatus; sub-normal Schwabach with positive Rinne and high-grade deafness for speech indicates a cochlear disease. Schwabach's test is not reliable with one-sided affection of hearing, or with double-sided disease in which there is much difference in the degree of disturbance on the two sides.[1]

## General Summary.

The chief problems of the paper[2] may be summarized as follows:

I. The definition of mental tests in general and their relation to experimental psychology. The position was taken that there is a very clear distinction between tests and psychological experiments proper, in that tests are rapid, approximate measurements applied to miscellaneous persons for practical purposes; while psychological measurements are made with refined methods, on practised observers, for scientific ends. A psychology of individual variations will doubtless be developed, but it will depend, the writer believes, on investigations of the latter rather than of the former sort.

II. The literature of mental tests. The movement was found to have arisen in Galton's anthropometric measurements. A psychological trend was imparted to it by Cattell, Jastrow, and the Committee of the American Psychological Association, so that mental tests and statistical manipulation of the results of measurements came to be regarded as a new and promising method of psychological investigation. This, the writer holds is a wrong conception. Titchener's article of 1893 called attention to the real separation of psychology and mental tests. The distinction which he made has come gradually to be accepted. This paper urges explicit recognition of the distinction, and acknowledgment that mental tests are not

[1] Politzer: *op. cit.*, 121.

[2] The first part of the paper, Chs. I-III, appeared in this *Journal* for Jan., 1904.

a part of psychology, but, as has long ago been said, are properly mental anthropometry. Such a distinction ascribes to tests only practical extra-psychological ends, and this is the standpoint taken by the present writer. The problem in hand becomes then, the description of anthropometric tests of audition. The problem naturally falls into two parts, general hearing tests and tests of musical capacity. To these, diagnostic tests were added to make a complete treatment of the subject.

III. General hearing tests were treated in two parts: (A) Speech tests, in which, after an examination of the problem, whisper-tests, employing series of number-words, were selected as the most feasible, and an improved method of testing was suggested; and (B) tests employing mechanical sounds, the watch, acoumeter, audiometer, and forks. With the latter division were considered tests to determine the lower and upper limits of tonal audibility. In general, acuity of hearing should be measured by speech tests, though mechanical tests form a possible substitute when convenience requires their use. In the appendix to this section, there are presented the results of experiments in the use of speech in auditory testing. The traditional method of extreme range in speech tests is shown to be unreliable, and a new method of degree of accuracy is proposed.

IV. Tests of musical capacity. Musical perception is too complicated to be tested directly as a unitary experience. Several sub-factors, however, were found to admit the application of tests: I. Pitch discrimination, measured by the adjustable pitch pipe, forks, or roughly, by the piano; II. Simultaneous tonal combinations; III. Rhythm and phrasing; IV. The affective reaction upon music; V. Miscellaneous methods, including a series of questions. The results of tests upon all these factors, taken together, give a basis for an approximate judgment of musical capacity.

V. Diagnostic tests. This section treated tests used in otological practice to localize and identify aural disturbances and diseases. The tests considered include: I. Diagnostic speech tests. II. Tests for integrity of tonal scale; III. Deafness; IV. Pitch difference between the two ears; V. Special tests devised by Weber, Rinne, Gellé, and Schwabach.

It is unnecessary to say that most of the diagnostic tests are of significance to the practising aurist alone. The tests of general practical value which have been stated are those of Chapters III and IV, anthropometric in nature, measuring the two important auditory functions, perception of speech and perception of music. These two sets of tests are submitted as contributions to what the writer believes should be termed mental anthropometry.

# MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY.

Communicated by EDMUND C. SANFORD.

## XVIII. COUNTING AND ADDING.

By L. D. ARNETT, Late Fellow in Psychology.

The simple arithmetical processes offer an attractive field for psychological study, not only because they form a definite group of highly developed and characteristically human mental activities, but, also, because a clear understanding of them might be expected to throw light upon a branch of education to which many weary hours are devoted. The aim of the present study is to examine some aspects of two of these processes, as they exist in the minds of educated adults.

### I.

### COUNTING.

The most fundamental of all the arithmetical processes both mathematically and psychologically is counting. This has been described by Hall and Jastrow as "the matching, or pairing, or approximative synchronization of the terms of two series of events in consciousness."[1] These two series are the series of number names and the series of objects to be counted. In order that counting may begin it is necessary that the series of number names shall first be well established mentally—in fact shall have become actually automatic, so that when once started it may be trusted to run off of itself, leaving the attention free to supervise the pairing of the terms in this series with those in the other. It is therefore natural that a child in learning to count first masters the number names as a purely linguistic feat and only later comes to apply them in actual enumeration.[2] The series of number names once mastered, the child

[1] Hall and Jastrow: Studies of Rhythm, *Mind*, XI, 1886, 55-62.

[2] This suffers an exception in the case of the numbers one, two and three, the groups corresponding to which are so early and so easily discriminated that the names for them may be learned like the names of things; and the ability to count objects within that range may thus come at practically the same time as the mastery of the number names. A little girl of five years to whom the writer gave a number of lessons in counting knew at that age, without any specific instruction, the number names one, two, three, four, five, eight and nine. She could count three objects correctly and sometimes four, but could do nothing beyond this.

is in a position to learn, and gradually to associate with each number name, the special qualities of the group to which it corresponds; so that, for example, "nine" shall upon occasion mean (or be associated with) three times three, or four plus five, or the square root of eighty-one, or any of the other relations in which it may occur.

In educated adults the number series has, of course, long passed the learning stage and become extremely automatic, and the readiest point for study of it is in its application under various conditions. Two such have been especially regarded in this study: (1) the counting of groups of visible objects at different distances from the eye, and (2) the counting of series of irregularly recurring clicks.[1]

In general it may be expected that anything that interferes with the equable succession of either the inner or the outer series, or that interferes with their co-ordination will be a hindrance to counting. In the first of the cases to be considered the trouble lay chiefly in the outer series, in the second in the habitual uniformity of the inner series.

*The Counting of Visual Objects.* The objects to be counted were small labels, colored black, one-half by three-fourths of an inch in size and one inch apart, and were placed in a horizontal row on a level with the eye of the observer. They were counted first singly, from left to right, then from right to left, then in groups of twos, threes, fours and fives, from left to right and from right to left as before. The distances at which the observer was stationed were, in different series, nine, fifteen, twenty-four and thirty-six feet. Two observers worked at the experiments, their task being not only to do the counting but also to note what might be discoverable with regard to the psycho-physiology of the process. The number of labels counted each time was about thirty, the observer never knowing the exact number beforehand.

It was observed in the counting, as might be expected, that the members of the number series were usually spoken, if not aloud, at least mentally, as the eye came to rest on one label (or group of labels) after another, the sudden stop and some slight muscular emphasis bringing the co-ordination to consciousness.

The counting of a series of small black labels arranged as in this case against a smooth light background, with no points of orientation except the spots themselves, brings into relief every method and device of the organism to keep the place as the eye passes along the line. One observer for example found himself taking notice of marks in the wainscoting three feet below the

[1] This sort of counting was suggested by Binet and Henri as a test of attention. *L'Année psychologique*, II, 1895, 446.

labels as points of orientation. At a distance of nine feet the labels could be counted with ease and certainty; at fifteen feet there was some difficulty which was increased still further at the greater distances. At thirty-six feet there was a tendency to pass beyond the label desired, and this became a fruitful source of errors in the count. Another source of error was the involuntary wink.

An interesting practical question is that of the most economical manner of counting. Counting by twos was felt to be a little more difficult than counting by ones, but the time of counting a given total of labels was, of course, decidedly lessened. The angular motion of the eyes was twice as great, and thus more clearly conscious, but more care was required to keep the place accurately, and upon this depends the counter's feeling of confidence in the accuracy of his count. Counting by threes is for the same reason more difficult than by twos. In counting by fours the observer usually found himself taking two twos, and in counting by fives a two and a three. The following little table, giving the results for one of the observers, based upon total counts of from 1,200 to 2,500 for each distance, shows that the time for counting a given total decreases as the size of the group increases, but less rapidly; while the accuracy is greatest in the double and quadruple group. As accuracy is usually the chief desideratum in counting, the count by twos would seem preferable under circumstances similar to those of these experiments.

| Counting by | Average time in seconds | Average of errors |
|---|---|---|
| ones | 20.4 | 5.0 |
| twos | 14.0 | 2.0 |
| threes | 11.5 | 3.8 |
| fours | 10.7 | 2.3 |
| fives | 9.6 | 3.5 |

The results for the second observer agree fully with these. A greater degree of certainly was felt in counting from right to left than in the reverse direction, but the reason of this was not discovered.

*The Counting of an Irregular Series of Clicks.* The apparatus for providing these clicks consisted of two pendulums of slightly different rate arranged with mercury contacts at the central points of their arcs. These were so wired that the transit of either pendulum across its drop of mercury caused the armature of a small electro-magnet to strike against its cores with a single sharp click while the sound of the back stroke was, by suitable means, eliminated. The movements of the armature were inscribed at the same time upon a revolving drum and thus objectively recorded. When both the pendu-

lums were set in motion at the same time they produced a set of clicks separated by intervals of varying length, the whole set recurring, after a number of seconds, according to a fixed system. The rate of the pendulums was such that, if the clicks had occurred at equal intervals, the counting would have been extremely easy.

The task of the observer was to count the clicks to the best of his ability. This he did silently and without seeing the pendulums or the recording stylus. Practice was continued until he could count with such a degree of certainty that, if he made an error, he could tell just where it occurred and explain the cause. The difficulty lay in co-ordinating the inner series of number names, which has a simple and uniform advance, with the procession clicks which were now crowded close together in pairs and now single and more widely separated. About thirty clicks were counted in each group, the observers, of course, being ignorant of the actual number given. Usually from fifeeen to seventeen groups were counted at a sitting. Four gentlemen, students in the University, served as subjects. They usually made many errors at first, but after five or six days felt certain of their counts and were justified by the record. Co-ordinating two such series involves either the complete giving up the serial character of the inner series or the remodeling of its rhythm to fit such as may be found in the outer series. Learning to count successfully meant, on the subjects' part, learning to hold themselves securely in readiness to record (*i. e.*, to count) instantly the one click or two clicks as they occurred, and then to be ready for the next. After some experience the observer mastered the nature of the recurrences in the series of clicks and knew pretty well when to expect either one click or a pair.

The point of chief interest in this experiment is the emphasis which it places upon the simply rhythmic character of the automatic inner series. The counting automatism, like all others, tends naturally toward a uniform periodicity, and special training is necessary for the acquirement of any other. This training in the case of counting is training of the mechanisms of inner speech, and is no doubt largely motor.

*Counting in General.* The psychical counting mechanism is in its operation essentially like the common mechanical counters in use in laboratories and elsewhere. It is so arranged that with a proper succession of (generally) like excitations to action it will bring forth its characteristic series of numerical symbols—the series of number names in inner or outer speech. In the excitations that cause it to operate an important element, if not the chief one, is voluntary movement, which serves as a signal for its action, or possibly in some cases, causes the

latter by a direct overflow of energy into the speech centre. The importance of this factor is evident in the need of special fixation of the eyes in visual counting, and of accommodating the rhythms in counting clicks. It is evident also in the inclination to touch with the finger actual objects to be counted or tell them off on the fingers, or by pencil strokes, or in some other way, in ordinary counting. Indeed it may be questioned whether counting would be possible at all without at least some trace of voluntary movement to put the psychophysic mechanism in operation. It is certain that counting would be immensely more difficult and carry with it less confidence of accuracy.

## II.

### ADDING.

I have sought to get light upon the process of adding in two ways; first by getting a number of gentlemen to add columns of figures for me that I might study their methods of adding both objectively and by the aid of their introspections, and secondly, by accurately timing the addition of certain pairs of numbers in the case of myself and another subject.

*On the Adding of Columns of Figures.* In this first group of tests eight gentlemen assisted me, with one exception students in Clark University, though not of the mathematical department. Each usually added for an hour at a time with a few minutes pause at the end of the first half hour. The figures to be added were presented in columns of twenty-seven figures each, fifteen columns to a sheet. The adding was done aloud and the partial sums were set down by the experimenter (generally myself) as fast as they were announced by the subject. At the end of each column both subject and experimenter went over together the series of sums and the subject explained any peculiarities of the results. In this way nearly 200 columns were added.

The most striking feature of the experiment was the variety of procedure in adding. Some of the subjects, and these were probably among the most rapid and least liable to error, simply added digit to digit all the way up the column, rarely if ever going out of their way to form combinations of the figures lying just ahead before adding them to the sum already reached. Others, on the contrary, rarely missed an opportunity to assist themselves in this way. As an example of the methods, I may cite the work of subjects C and E, which is typical of the extreme types.

*C* added 810 digits and announced 519 results. During the adding he used 213 combinations, 171 of which were of two digits, and 42 were of three, with an occasional one of four

digits where they were small. The combinations used were as follows:

| Digits grouped to form | No. of times. | Digits grouped to form | No. of times. |
|---|---|---|---|
| 3 | 4 | 10 | 36 |
| 4 | 6 | 11 | 18 |
| 5 | 18 | 12 | 27 |
| 6 | 10 | 13 | 13 |
| 7 | 10 | 14 | 9 |
| 8 | 16 | 15 | 4 |
| 9 | 21 | | |

As to the number of each, there were 8(5+7) combinations; 6(2+8); 6(3+2); 6(4+1); 5(8+4); 5(4+7), etc. This shows in some measure the relative frequency of grouping certain digits.

*E* added 840 digits and announced the same number of results. He named the first digit and added each digit separately and was so rapid in announcing his partial sums that it was difficult for the experimenter to record them.

The combined results for all the subjects were as follows: Of the 5,295 digits added, 2,975 were added singly and 974 combinations were used. Of the latter, 914 were of two figures and 60, of three or more figures. Of the frequency with which the groups were used, the following table is a representation:

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 20.9% | were | groups | the | sums | of | which | equaled | 10. |
| 13.6% | " | " | " | " | " | " | " | 9. |
| 10.4% | " | " | " | " | " | " | " | 5. |
| 9.5% | " | " | " | " | " | " | " | 8. |
| 9.4% | " | " | " | " | " | " | " | 11. |
| 8.4% | " | " | " | " | " | " | " | 7. |
| 8.3% | " | " | " | " | " | " | " | 6. |
| 5.9% | " | " | " | " | " | " | " | 12. |
| .etc. | | | | | | | | etc. |

As shown above, the number decreases as the interval increases, either way, from ten; five, possibly because of its place in the decimal system, is an exception.

The tendency in grouping seems to be to add primarily by tens, then to add by nines which is ten less one. The combinations that form eleven and eight appeared in the adding with nearly the same frequency. The subjects report that eight is often added as ten less two, and that eleven is nearly always added as ten plus one.

The cause of such errors as were made seemed to lie very frequently in the influence of some preceding figure still delaying in the mind. Bergerstein[1] in his studies of Vienna school

[1] *Zeitschrift für Schulgesundheitspflege*, Vol. IV, pp. 556-560.

children, found this to be one of the most common causes of error, both in addition and multiplication. Sometimes the tens were not carried or were carried too far. Excessive combination of the numbers seemed to involve error, in that too much was carried in mind to allow of clear holding of all the elements.

When uncertain, the subjects often tried to verify their results as they went along. The only means of verification used, except that of re-adding, was manipulation of the digital combinations. When there was doubt, for example in combinations such as 37+6, the poor adders would take three from the six and combine it with the 37 to make 40 and then add the three; and in some cases they even counted by units. The more proficient ones would in such a case refer to the digital combination 7 + 6, and getting the result as thirteen, would carry the ten almost mechanically. But no hard and fast line of distinction can be drawn, for at times the best adders would break up the numbers, and the poor ones would refer to the digital combinations, though they could not always recall them.

The unanimous opinion was that it is more difficult to make large than small steps in the adding, *i. e.*, to add the larger digits. Further evidence of this appears in the fact that when the subject was not feeling well, or otherwise was not in the best condition for adding, he made fewer combinations, sought only the easier ones, and if there was any inclination to count, to separate numbers, or to take them out of their natural order; all these devices were resorted to. The reference to the digital combinations was most frequently in visual terms. One subject said that he could "see the work within three or four feet in front of him." The tactual and motor senses as aids in combining have probably a deeper meaning, but are less readily observed in rapid work, except as the hand or pencil is used to keep the place which is perhaps itself an assistance.

Another subject, a high school student, a poor adder, would seek every opportunity to separate the numbers and make easy combinations, and where the numbers were small they were added as units. The gentlemen that made few combinations would most frequently take numbers out of their order to facilitate easy combinations. The seemingly mechanical process of the rapid adders would lead them to forget their exact position with relation to the tens, thus making them liable to err. Excessive grouping of numbers is a loss of time in some cases, as it causes a hesitancy in adding, time being also required to decide what groups are best to make. But on the whole the results would seem to indicate that the subject who can use a few combinations judiciously has the advantage in the process.

From observation in this work it would seem that the trained

apprehension of the sum of two digits is a process not unlike the recognition of the proper pronunciation of a word. When we see a word, we rarely think of the sounds of which it is composed, but grasp it as a whole. To produce a similar degree of proficiency should be the aim of the teacher of elementary arithmetic.

*On the Rate of Adding Certain Combinations of Digits.* In this section the writer ventures to present the somewhat meagre results of a chronoscopic study of certain special additions. Incidentally determinations were also made of the reading time for the single numbers from 0 to 12. Both series of experiments were made in the usual way with the Hipp chronoscope and mouth key. The numbers (about three-quarters of an inch high) were displayed at the instant of starting the chronoscope by means of a Cattell fall-chronometer used as a falling screen. The uniformity of the chronoscope was tested at the beginning and end of each sitting by means of a large regulating pendulum; and of the subject's reactions only such were preserved in the protocol as at the time satisfied both subject and experimenter as having been made under standard conditions both internal and external.[1]

Two subjects (A and B) participated in the reading experiment and furnished an average of twelve records each for each of the thirteen numbers read. The times for reading these numbers range, for A from $281\sigma$ to $380\sigma$, and for B from $255\sigma$ to $286\sigma$, and give the following orders of quickness in reading:

For A: 0, 8, 1, 10, 2, 11, 7, 3, 12, 9, 5, 4, 6.
" B: 8, 4, 3, 2, 1, 0, 5, 10, 11, 6, 12, 7, 9.

If we separate each of these series into a quicker and a slower half (above and below the median term—7 for A and 5 for B) the following points of agreement may be noted: First, 0, 1, 2, and 8 for both subjects come before the median (*i. e.*, are quickly read), while 6, 9, and 12 follow the median and are slowly read. The medians themselves (7 for A and 5 for B) belong, each in the case of the other subject, to the slower group and may well be classed with the slower numbers. The remaining numbers 3, 4, 10, and 11, show different results with the two subjects and must be left doubtful. There is of course little probability that these differences are due to differences in the ease of recognition of the individual number symbols or to differences in the facility of the associative processes between the symbol and its name. A much more likely cause is to be found in a varying difficulty of enunciation or in the way in

[1] The chronoscope values have not been reduced to absolute time values as the chief interest in the determinations is relative.

which the mouth-key operated in case of the different movements required in speaking the different names.

The tests on adding followed those on the reading of numbers. Subject A added the combinations found in the tables of 2's, 3's, 7's, and 8's from 0 to 12 complete (2+0, 2+1, 2+2 . . . . 2+12, 3+0, 3+1, 3+2 . . . . 3+12, etc.), presented in irregular order to an average of eleven times each. B added certain selected combinations from the same tables to an average of ten times each. A's times range from 479$\sigma$ for 2+8 to 655$\sigma$ for 8+12; B's times from 377$\sigma$ for 8+10 to 824$\sigma$ for 8+12; but in A's case nearly four-fifths of the times fall between 479$\sigma$ and 579$\sigma$, and in B's case three-fourths between 419$\sigma$ and 519$\sigma$. The fact that the range of variations in these adding-times are on the whole so little greater than those found for the simple reading of numbers, points out at once the impossibility of making determinations on the relative ease of adding the different combinations of numbers without very much more prolonged and careful experimentation. In fact the introspection of the subjects testified that in many cases the sum was reached by an association which seemed practically as simple as that of reading (*i. e.*, naming the number symbols), and one therefore in which specific differences in time would hardly be expected to appear. It is interesting to notice, however, that the characteristic lengthening of the time for the adding, which was originally the more complex process, still persists, though this again might disappear with sufficiently prolonged practice.

One or two relations are to be found in the full tables with sufficient definiteness to warrant mention. With but a single exception (19 cases in 20) it seems to be a relatively slow process to add to the larger numbers 9, 10, 11 and 12, but how much of this is associational, and how much to be credited to difficult enunciation in the 'teens cannot be said. In seven cases out of eight it seems to be a relatively quick process to perform additions resulting in 10,—but "ten" was again an easy name for one of the subjects to say. The sum of a number added to itself was often more quickly announced than the median combination in the same table—three cases out of four for A, and two out of four for B, with a third standing next the median on the slower side. The instance of failure for A was 3+3 for which he was continually beset to announce the product instead of the sum. The greater ease of such combinations was repeatedly noticed in A's introspections.

In other instances the dicta of introspection were little or not at all supported by the chronoscope. A reported, for example, that even numbers seemed easier to add than odd ones, but the tables show little evidence of it. Both subjects were frequently

conscious of taking the figures in such an order as to bring the larger digit first if there were any considerable difference in size, thus 8 and 3 were added by preference as 8+3 even when presented as 3+8, but the tables again show no lengthening of the time to correspond to such a re-arrangement. In fact one cannot but ask himself whether the conscious re-arrangement may not have followed, rather than preceded, the announcing of the sum—the associative reaction having taken on a characteristically motor form.

# THE PLACE OF MENTAL IMAGERY AND MEMORY AMONG MENTAL FUNCTIONS.

By FRED KUHLMANN, PH. D., Assistant in Psychology, Clark University.

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The ability to learn has been made a criterion for the existence of consciousness. And in these instances the term 'learning' is used in the individual sense but may include hereditary maturation of functions during the life of the individual, as well as the functions that are learned in the narrower sense. The methods of the acquisition of function are various, and not all of them belong to the individual. It may be profitable to mention at least what from the present point of view may be called the racial method of learning. Whether the evolution of animal organisms takes place through natural selection and this alone, or by other means as well, the fact that an evolution has and is taking place with most animal species will perhaps not be denied. In the experience of a species with nature and with itself, like that of the individual in his limited environment, it is making progress in the ability to deal with that environment and nature. It is making adaptations. And this may, in the wide sense, be called learning. It has indeed by some been designated by the term 'organic memory.' As far as dealing with the problems of life is concerned the result of this sort of learning is for the individual not different from any other sort. The individual of each successive generation, in a very general way, finds himself better able to cope with the difficulties that nature presents to life than the individual of the preceding generation. And this is the function of individual

acquisitions. It is an improved ability to do and to live. But, excluding the inheritance of acquired characters the individual has in this case had no hand in this progress. Nature has produced the variations, and has done the selecting of the best by killing the others. Or, if we choose to regard the variations as produced by the individual, the latter has had no part in their production through any influence of consciousness or through the influence of those of his activities that need a schooling for their perfection. Consciousness is not a necessary factor in this mode of progress, nor is functional activity. It is strictly within the realm of hereditary improvement. In the more complex and highly organized nervous system of man the human race shows the results of a schooling from nature far in advance of any that has been received by any of its competitors. It has learned to react to its environment in ways that make it get much more out of nature, make a much more extensive use of it than lies within the possibilities of any other species. The greater flexibility of the human nervous system, its greater capacity for individual acquisitions, or learning in the narrower sense, is one of these marks of superior training in Nature's school. But in the prominence of this fact, the greater educability of man, we are apt to lose sight of his other superior hereditary endowments which are equally the result of a natural evolution. And when we contemplate the scope and significance of what a man can learn in a lifetime, and the individual differences in this respect where the natural endowments have been more or less equal, we are still more apt to forget the rôle that the latter are playing. Nature's methods in producing these results have been slow, and the ages of history required in making man make it seem like the employment of a clumsy procedure, against which the results of the individual's education appear in a false light, and a light which is borrowed at that. In the further consideration of this general topic this fact should be constantly borne in mind. For unless this is done we lose sight of the real relations between the various factors that enter into making the human mind superior in adaptability and in the ability to learn.

We may turn now to the methods of learning, when the term is used in the individual sense. How can the individual make acquisitions that are useful in life? As was stated a few moments ago, the ability to learn has been made the criterion for consciousness. 'If the individual cannot learn to react more profitably to stimuli as the result of his experience we have no evidence for the existence of a conscious life. And *vice versa*, without consciousness the individual cannot thus profit by his experience.' Before accepting this assumption we should at least raise the question of the possibility of the individual's

ability to learn where consciousness does not enter as a factor. A purely neurological adaptation of a reaction to a stimulus is at least conceivable; and why should not the same be possible, also, even where there is no nervous system? A physiological, or, ultimately, mechanical explanation alone could be made of such a phenomenon. But why should not such a mechanical process be possible and sufficient to produce the results in question? Such a process we conceive to be associated with the activity of neural tissue concerned in conscious life. What is there, in our theory, about this physical process that may not be also present in every living cell? This is a theoretical question and no answer can come from observed fact, but must be derived, if obtained at all, from inference. We can determine the ability to learn in animals, but we cannot tell, directly, what kind of a life this implies.

But there is another aspect in the assumption that consciousness and the ability to learn go together and that the latter may be made a criterion for the former, in which our answer need not be entirely speculative. It is implied that in learning from experience a *conscious* use is made of that experience. 'There is some sort of conscious memory of the experience and it is through this that the adjustment in the reaction to a stimulus is made. It is an intelligent reaction or behavior, as distinguished from the instinctive, the reflex, and the habitual.' The view that this must necessarily be so is, however, hardly tenable. There are types of learning in the human organism that do not consist of a conscious adjustment of the reaction. These may be for the most part instances in which the hereditary maturation of structure is involved, but in any case perfect functional activity is preceded by a period of imperfect functioning without which the later perfection is not acquired. Hereditary structural growth alone is not sufficient, but it requires the stimulus and activity for functional development. In this sense, there is probably an education of all of the involuntary muscles of the body. The sympathetic nervous system has a variety of functions to perform, and the detail of its duties must undergo many changes during the period of growth from birth to the adult life. On the functional side, we know very little at present of just what adjustments must be made in the activities of the different organs of the body. On the structural side, we know that most of the viscera, glands, and other organs show no regular course of development. This is reason for believing that the functional adjustments also are greater and more varied than have been yet determined. The first radical change comes at birth. A digestive system, with quite a series of associated glands and organs, must now learn to do its work. It is a learning process

in every sense of the word excepting that a conscious guidance of the activities involved is absent. The digestive, assimilative, and secretory processes of the infant are quite a different matter from those of the adult. The salivary glands are at birth practically inactive. The movements and the secretions of the stomach and the intestines change. The same is equally true of the functions of the liver and spleen, and of the kidneys. A second radical change at birth takes place in the circulation. With the cutting off of the placental circulation the blood takes, in part, a different course, and an adjustment in the circulatory mechanism is required. Structurally and functionally the heart and arterial system undergo many variations from birth to maturity. Breathing with the infant has some well marked characteristics at birth, and comes within the course of a few weeks to take on a more or less permanent character. But the function is not perfect at first. The lungs do not entirely expand, and may not do so until the end of the second week. The breathing is irregular. One lung may be at times inactive. The breathing has not its later normal rhythm, except during sleep. A period of tuition is required in order to attain the ability to do its perfect work.[1] Instances of this sort might be multiplied, but this is perhaps not necessary for our present purpose, which is only to call attention to the fact that there are many cases in which the various functions of the sympathetic nervous system and the involuntary muscles require considerable readjustment in order to meet the complex demands of a growing organism. Many of the cases that might be cited under this class require perhaps no more than a purely mechanical readjustment; mechanical in the sense that no period of more or less gradual functional development precedes the perfect form. But many other cases clearly do require such a preliminary schooling.

Better known examples, however, of functional development where conscious guidance is not involved are found in some of the early reflexes of semi-voluntary, and voluntary muscles. Here, too, it will not be necessary to state details. A large group of these reflexes is connected with the muscles of the face and its sense organs. The natural reaction in the facial muscles, especially to tastes and odors, pleasure and pain, is quite uniform in the adult and in early childhood. But they are not present at birth, and most of them show a distinct though brief course of development. Thus there are the perfectly reflex facial expressions in response to bitter, sour, sweet; to pleasant and unpleasant odors, to pleasantness and

[1]Oppenheim: The Medical Diseases of Childhood, N. Y., 1900. Holt: Diseases of Childhood, Phila., 1899.

unpleasantness in general, which the child *learns* to make. More definite still than these are the reflexes of some of the eye muscles. The muscles that move the eyes have to learn to act together in the delicate harmony of movement required of them in binocular vision. The ciliary muscles of accommodation must learn to adjust the lens for near and far vision. The iridian muscles must regulate the size of the pupil in response to increase and decrease in intensity of light, and to changes in accommodation. All three sets of ocular muscles must learn to act together in the complex act of vision, a thing of which they fall far short at birth. The wink reflex of the eyelid, also belongs to this group. These instances of the development of reflexes are cases that perhaps no one will question with reference to the absence of a conscious guidance of the activity. Whatever sensations arise from the stimuli and from the contraction of muscles and the movements of the sense organ, and whatever the influence of these sensations may be in the adjustment of the reaction, we cannot believe that the child at this age makes voluntary use of these sensations in a voluntary guiding of these fine and quite complex co-ordinations. There are also many other co-ordinations that are of the reflex order, and are made during early childhood, that may not so clearly belong to the above type, but which yet on the whole resemble them closely These are the various early co-ordinations of the voluntary muscles. Here belong first those involved in the development of space perception, *e. g.*, the reflex turning of the head in the proper direction when a sound is heard; the reaching out towards objects when seen within reaching distance; and the very numerous co-ordinations between stimulation of the skin and arm and hand movements, by means of which stimuli on the different parts of the body are localized and the child comes to what we are accustomed to call an 'acquaintance with self.' Still further removed from the first group in their less reflex character and in the degree in which voluntary control plays a part in their acquisition are the later co-ordinations among voluntary muscles, such as learning to walk, and learning the co-ordinated use of arms, hands, and fingers, together with the other muscles of the body, as they come into play in the ordinary activities of daily life. These co-ordinations are practically all acquired during early childhood and they all approach the character of the reflex, at least so far as the interconnection of the movements of the separate groups of muscles is concerned. The will plays a part only in initiating or inhibiting the series. Consciousness plays a part in their acquisition in repeatedly initiating the effort to perform them as wholes, but we could hardly go further and say that the child is aware of the separate movements involved in such com-

plex acts, conceives of them beforehand and consciously guides them in their required succession.

In the learning of these activities we are touching upon a method of learning that is not peculiar to early childhood, but is shared by all adults in this sort of activity. Before passing on to the next topic we may pause, therefore, for a moment, to consider what sort of memory is involved in this kind of learning. It is the kind of learning of which we say that we learn by doing, and not by reasoning it out. We are conscious of some of the factors in the total complex ; we are conscious of the purpose of the whole, of the effort we put into it, of a very general and vague sort of voluntary directing of our movements and mental processes; we have perhaps sensations resulting from the movements, and we are aware of the accuracy or inaccuracy of our results, of our success or lack of success in what we are trying to do. But on the whole the progress in our skill does not come about *mainly* through conscious guidance, through conscious correction of previous errors. We acquire skill in writing, in gymnastic exercises, in swimming, skating, rowing, in learning various games, by *practice*, by repeated, more or less blind efforts, in which we very seldom recall a previous error, which we then consciously avoid, or by foresight of the correct procedure carry out our conceptions of it with attention to the details in the activity. The progress that we make in our ability comes about through a *neural* adaptation, in which the organism is not burdened with the higher processes of memory and intelligent action. The adaptations take place unconsciously; we become aware of them only after they are made, and have little to do with their making. To be sure we need our sense organs in these activities, and the deduction might be made that memory images of previous activities, must enter into the learning process. But as a matter of fact, they rarely, and in some instances never, do. The memory involved in this sort of learning is not conscious memory. It is neural only. It is learning in a conscious individual and consciousness takes a distinct part in the activity in which progress in ability is being made, but the learning does not take place mainly through conscious guidance of corrected movements, etc. It is a method of learning distinct from those described before and from the method to be considered next. There will be occasion in other connections to recur to it again for further consideration.

We come now to consider the memory image and the mental image as factors in the ability to learn. The existence of the type of learning just described demonstrates the possibility of progress in the ability to do certain things without the guidance of conscious memory, or of mental imagery. This fact, how-

ever, has never been taken into consideration in the efforts to determine what sort of mind was implied in the ability to learn in the case of lower animals. The ability to learn has by some not only been set up as a criterion for consciousness, but also as a criterion for the memory image. In the next few pages I shall attempt to state briefly the discussion that has arisen on this point.

Much of our older and even present day psychology holds that perception and the memory image mutually imply each other. 'Perception is a very complex process in which the memory of much previous experience is included. It is really a sensation simultaneously supplemented with mental imagery of the past.' According to this view the question as to the priority of consciousness and the memory image, or the necessary implication of the latter in the former, is settled. Perception implies the memory image and hence there can be no consciousness at all without the presence of the memory image. Possibly this view has influenced some of those who have accepted the ability to learn as a criterion for consciousness. Loeb makes this criterion 'associative memory,' which he defines as 'the ability to learn or the ability to profit by experience.'[1] If there is associative memory there is consciousness. He is not very explicit as to what is to be included in associative memory. The definition just stated, which he usually uses, does not define it with reference to the kind of memory meant. From some other statements, however, it becomes quite clear that he uses the term in the more usual sense. He means by it mental images that result from past experiences. Whether these mental images are complete memory images, that is, have included in them the recognitive elements that make the subject aware that his imagery relates to his own personal past experience, which he localizes with reference to time and place, cannot be made out with certainty from his descriptions. A few quotations will give his point of view, so far as he states it explicitly. "By associative memory," he says, "I mean that mechanism by which a stimulus brings about not only the effects which its nature and the specific structure of the irritable organ call for, but by which it brings about also the effects of other stimuli which formerly acted upon the organism almost or quite simultaneously with the stimuli in question. If an animal can be trained, if it can learn, it possesses associative memory."[2] In another place he says: "We may conclude that associative memory is present when an animal responds upon hearing its name called, or

[1] Physiology of the Brain, N. Y., 1900, pp. 12, 214, 218.
[2] *Ibid.*, p. 12.

when it can be trained upon hearing a certain sound to go to the place where it is usually fed. The optical stimulus of the place where the food is to be found and the sensations of hunger and satiety are not qualitatively the same, but they occur simultaneously in the animal. The fusion or growing together of heterogeneous, but by chance simultaneous, processes is a sure criterion for the existence of associative memory."[1] Again: "The odor of a rose will at the same time reproduce its visual image in our memory, or, even more than that, it will reproduce the recollection of scenes or persons who were present when the same odor made its first strong impression on us. By associative memory we mean, therefore, that mechanism by means of which a stimulus produces not only the effects which correspond to its nature and the specific structure of the stimulated organ, but which produces, in addition, such effects of other causes as at some former time may have attacked the organism almost or quite simultaneously with the given stimulus."[2] In the last quotation he defines what he means in the first by the 'effect of a stimulus' which is reproduced not by its own stimulus that first produced it, but by another that was once associated with the former and is now present. He means, apparently, the memory image of the previous stimulus. Thus, implicitly at least, he holds that the ability to learn involves the function of the memory image.

Lloyd Morgan is in essential agreement with Loeb, although he points out some distinctions not considered by Loeb. He accepts the ability to 'profit by experience' as a criterion for the existence of consciousness in animals.[3] But for him there may be two grades of consciousness that are fundamentally different with reference to their survival value. Organic behavior may be accompanied by a mere *sentience* only, a conscious accompaniment that does not influence or guide behavior. Or it may be accompanied by *effective* consciousness, a consciousness that does guide behavior. The ability to profit by experience is a criterion only for effective consciousness. He raises the question as to the kind of memory involved in this, and rules out conscious memory, that is, the complete memory act, from playing a part in animal behavior. There is, first, no localization in time. The animal does not remember his experience as belonging to a definite part of his past with reference to time.[4] Such a memory would have little or no practical value for animals, and there is no basis for assuming its presence. There is only a re-instatement, a re-presentation of the past experience in terms

[1] *Ibid.*, p. 218.
[2] *Ibid.*, p. 214.
[3] Animal Behaviour, Lond., 1900, p. 42.
[4] Introduction to Comparative Psychology, Lond., 1900, p. 122.

of mental images and their previously associated affective processes. In the chick's second experience with the caterpillar "the *presentative* visual stimulus now calls up *re-presentative* elements, motor and gustatory; and these place the situation in a wholly new aspect. They give it what Dr. Stout terms 'meaning.'"[1] In another statement he refers more definitely to the distinction in point. "We may now substitute for the word remembrance," he says, "the more technical term re-presentation. Profiting by experience, regarded as a criterion of the presence of effective consciousness, involves re-presentative elements in the conscious situation which carry with them meaning."[2] In a word, then, he assumes the mental image as a necessary factor in the ability to learn. If his terms 're-instatement,' 're-presentation' are to be taken literally, he excludes the recognitive elements involved in the full memory act. These are unnecessary factors for 'intelligent' behavior. In this he differs, apparently, from Loeb. Incidentally, too, attention may be called to the fact, that his effective consciousness implies that it guides consciously. It makes use of mental images and by means of them guides action. Learning for him can take place only through conscious guidance.

In a discussion on "The Genesis and Function of the Memory Image" Bentley takes a quite different view in regard to the relation of the image to the ability to learn. The mental image, which Loeb and Lloyd Morgan regard as the essential factor for the possibility of learning, Bentley considers unnecessary. And the recognitive elements in memory which Lloyd Morgan excludes as unnecessary, he regards as the only factor needed for the adjustment of behavior in accordance with experience.[3] They differ then as to what elements in the complete memory act are involved in learning. His view seems to be based upon one established fact concerning *human* consciousness, although he does not explicitly point out this relation. This established fact is that recognition of an object can, and sometimes does, take place without the presence of a memory image of that object as previously experienced. We are immediately aware of the object, in these cases, as a familiar one without our recalling that we have experienced it before. According to him the consciousness that is capable of mental images is a stage in advance of a simpler type in which mental images do not occur. The organism possessing the latter form, to be sure, has a memory, and is capable of profiting by experience. But it lives, and can live only in the immediate present.

[1] Animal Behaviour, p. 46.
[2] *Ibid.*, p. 47. Also Introduction, p. 122.
[3] The Memory Image and its Qualitative Fidelity. *Am. Jour. of Psy.*, 1899. pp. 18-23.

Its consciousness is all of the perceptual kind, and without the presence of a stimulus there is none. Perception and recognition are sufficient to do the simple business of its life, and the recognition is of the immediate sort. The mind that can *imagine* is of a higher grade, and learning with the use of mental images is quite a different method of progress. The perceptual consciousness involves the past experience of the individual. There is memory in this sense only. But as perception is psychologically simpler than recognition, so recognition is a simpler process than a memory that involves mental images. Since recognition is possible without mental images, and since no more than this is needed to account for the ability to profit by experience, the point at which the mental image comes in in the biological development of consciousness becomes a problem to be determined. A few quotations will make his view clear on this point. "So long," he says, "as a rough-and-ready attitude towards the world suffices for survival, images will not appear. It will only be after the non-present has been thrown up against the background of perception that its content will be conceived as possibility and then a warning,—and thence the way to prophetic adaptation (adaptation by the use of mental images) will be short."[1] "Thus cognition or simple recognition would govern reaction towards the immediate environment, and only a later and more complex stage would demand conscious previsory adaptation (adaptation through mental images). This stage would be the longer delayed because instinctive reactions are reinforced among some of the higher animals by a motor habit, set up through the individual's experience, which seems to mediate recognition without the addition of memory images."[2] "If we take the biological point of view, it seems altogether probable that the memory image was a comparatively late acquirement for the organism. A command of consciousness through active attention, that should abstract from the pressure of the immediate environment and hold an isolated sensation-complex, aroused by central excitation, is the prerequisite of the image. A highly complex nervous apparatus seems necessarily involved."[3]

Bentley does not attempt to describe further what kind of a mind it is that makes its adaptations without the use of mental images, but he considers what rôle the mental image plays when it does appear in mental evolution. This I shall take up in a moment. Thorndike approaches the same problem from the results of experimental studies on animals. He is in entire agreement with Bentley with reference to the ability to learn without the use of mental images. He also makes some further

[1] *Ibid.*, p. 23. [2] *Ibid.*, p. 22. [3] *Ibid.*, p. 18.

efforts to describe this sort of mind.[1] He classifies the methods of learning into three types: (1) Learning by trial and accidental success. (2) Learning by imitation. (3) Learning by ideas, where the situation calls up some idea which then arouses the act or in some way modifies it."[2] The first method is that of animals, or more correctly, of the animals he has studied. It does not involve the use of 'free ideas,' which term he uses instead of 'mental image.' The adaptation in the reaction to a stimulus is here brought about first by accidental success, and then a subsequent strengthening of the associative connections between the present sense impression and the correct reaction. The many reactions that are wrong at first are not so strongly associated with the corresponding sense impressions. The right reaction is followed by the satisfaction of a desire, emotional elements that give an advantage to the associative connection between this reaction and the sense impression. This connection is direct; the reaction is immediate. There are no re-presentations of past sense impressions and the past reactions with its results between the now present sense impression and the now correct reaction. Thorndike does not further compare this process of learning with the factors involved in memory, and recognition of the human mind, and it is not quite clear as to what he would include in it from this point of view. Taken as stated, this kind of learning is a simpler process than even Bentley's view implies. The animal does not react from the *recognition* of a situation that has come to be familiar to him through previous experience, but he comes to do so directly, without his knowing, or feeling why. The difference between the first and the later correct reactions is perhaps only that he finally comes to react more readily, has a stronger impulse to react in this direction immediately upon the presentation of the stimulus and the sense impression. From the animal's point of view, he simply comes to do it right, and that is all.

To make clear and more definite the differences in all these views as to what is involved in the ability to learn, we may take Lloyd Morgan's illustration of the chick learning to eat the 'good' and not the 'bad' caterpillars. The facts here are that the chick at first pecks indiscriminately at caterpillars that it does not like as well as those it does like, but comes finally to peck only at the latter. Loeb would seem to hold that after a little experience the sight of a particular caterpillar would produce a memory image of previous experience with this sort

[1]Animal Intelligence. An Experimental Study of the Associative Processes in Animals. Psyc. Rev. Mon. Supl., 1898. The Mental Life of the Monkeys, Psyc. Rev. Mon. Supl., 1901.

[2]Mental Life of the Monkeys, p. 2.

of looking thing, a memory image of another caterpillar like this and of how it tasted. Upon the basis of this the reaction is adjusted. If the memory is of a bad taste the caterpillar is left alone, etc. Lloyd Morgan would not attribute to the chick a complete memory image; no conscious memory. Upon the second sight of a particular caterpillar there is a representation of the previous experience, visual and gustatory. This is sufficient to adjust the reaction. But the chick is not aware that it has seen or tasted this sort of thing before. According to Bentley's view there is no re-presentation of past experience, visual or gustatory, at the second sight of the caterpillar, but there is a direct recognition of it as good or bad, as something to be eaten, or something to be avoided. It is a familiar thing that means to the chick, 'eat' or 'don't eat.' From this the proper re-action follows, as in Lloyd Morgan's case. Finally, Thorndike does not include even such recognition. The chick at first eats good and bad alike, with the results of satisfaction or dissatisfaction. From this experience the associative connections between the visual stimulus and the movements in eating become strengthened in the one case, weakened in the other. No mental images or recognitive processes are necessary for this strengthening or weakening of associative connections. The 'good' or the 'bad' caterpillar does not come to *mean* anything different to the chick than it did before. He simply comes to eat with a stronger impulse to do so in the one case, and with a weakened impulse to do so in the other. Thorndike attempts to describe the animal consciousness, and the animal's method of learning in general terms, and points out what seems similar to this in the human mind. This description I shall quote in full. "One who has seen the phenomena so far described," he says, "who has watched the life of a cat or dog for a month or more under test conditions, gets, or fancies he gets, a fairly definite idea of what the intellectual life of a cat or dog feels like. It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts *about* them or memories of how they looked at other times, or æsthetic judgments about their beauty; one feels no *ideas* about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connection of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all. This pictorial description may be sup-

plemented by an account of some associations in human life which are learned in the same way as are animal associations; associations, therefore, where the process of formation is possibly homologous with that in animals. When a man learns to swim, to play tennis or billiards, or to juggle, the process is something like what happens when the cat learns to pull the string to get out of the box, provided, of course, we remove, in the man's case, all the accompanying mentality which is not directly concerned in learning the feat. Like the latter, the former contains desire, sense-impression, impulse, act and possible representations. Like it, the former is learned gradually. Moreover, the associations concerned cannot be formed by imitation. One does not know how to dive just by seeing another man dive. You cannot form them by being put through them. One makes use of no feelings of a common element, no perceptions of similarity. The tennis player does not feel, 'this ball coming at this angle and with this speed is similar in angle, though not in speed, to that other ball of an hour ago, therefore I will hit it in a similar way.' He simply feels an impulse from the sense-impression. Finally, the elements of the associations are not isolated. No tennis player's stream of thought is filled with free-floating representations of any of the tens of thousands of sense impressions or movements he has seen and made on the tennis court. Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with animals. There is consciousness enough, but of this kind." [1]

In this passage there is pointed out a way in which the animal and the human method of learning, as far as the learning process itself is concerned, are essentially alike. In each case conscious memory, conscious guidance of action does not take place. What then is the function of the memory image where it does exist? The answer may now be brief. Without it consciousness must move within the narrow limits of the present. No memory of the past, and no expectation for the future can be there. Although adaptations can and do take place, as we have seen, they must all be made at the spur of the moment, in the immediate presence of the stimulus that has once caused a wrong reaction, and calls for a readjustment on the part of the organism. With the memory image, consciousness transcends the present, and recalls not only the past but penetrates the future as well. Indeed, the backward look of the memory image is its least contribution to life. This may add to its æsthetic enjoyment, but it is the forward look that carries with it the utilitarian significance. By means of it the

[1] Animal Intelligence, pp. 83-84.

organism may now look ahead, may decide upon and adjust its reaction beforehand, in the expectation of meeting the same situation again. To quote Bentley once more: "The primary use of the image, we surmise was to carry the organism beyond the limits of the immediate environment and to assist it in foreseeing and providing for the 'future.' Its function seems, then, to have been a prophetic one; it was a means to what we may term *remote adaptation*. . . . . When once the capacity for independent imagery has been acquired, the stress of existence will inform the image with significance. The significance will, undoubtedly, be vague for a time. The image will mean a situation which does not belong to the one immediately presented; its temporal situation will still be unfixed. Later, its extreme importance will attract the attention, and thus it will be reacted upon. As an element in action, it will become more and more teleological; it will become the headlight of consciousness, pointing before to an experience which is to follow, and which must be provided against."[1]

We have come now to the point in the biological evolution of consciousness where the mental image and memory become of conscious use. The individual has now a conscious record of his past. He can consciously recall his experience, can think about it, can see the errors in his behavior and can decide upon the proper mode of reaction to a situation that he has once met and expects to meet again. The memory image has truly furnished a means to remote adaptation. It has supplied the conditions for a method of learning that far exceeds any of the others so far considered. At this point we may raise the question whether the memory image has retained this supreme function in the further development of consciousness as existing in man, or if not, what place it now holds as a means of guiding human conduct. Have any other methods of adaptation been reached that exceed even the memory image?

In the consideration of these matters we come at once when we turn our attention to human life to some fundamental distinctions. The human individual of the present, at least, is capable of making use not only of the memory of his own experience but of the history of the whole race that has preceded him. Several new factors have come in to make this possible, and to change further the means and methods of guiding conduct. These factors are closely related to each other, but I shall attempt to state them separately under (1) the influence of language; (2) the limitations of our immediate sensory data; (3) the rôle of reason and science in life.

Perhaps no other one factor that belongs quite entirely to the

[1] *Ibid.*, pp. 18-19.

history of the *human* race has influenced mental imagery and memory as much as has language. Without the existence of language, spoken or written, no adaptation through the combined knowledge of the race was possible. Outside of what could be learned through imitation and other non-linguistic forms of suggestion the only source of information that the individual could make use of in consciously guiding his conduct was that of the memory of his own experience. And that experience could be held together only in terms of memory imagery, however abstract and remote from the original sensory data this might become. Memory of the individual's own experience stood supreme as a means to adaptation and of guiding conduct. With the introduction of language, especially written language, the conditions for radical changes were given. With respect to his own experience the individual can now cast this in terms of a system of symbols that are at any time convertible into the memory of the original, and which are vastly easier to preserve than the original memory images. The written record can be made to tell the story of his life, his individual acquisitions, all that he has learned that will aid in meeting the difficulties and the problems of life. The details of the contents of this record he need not constantly keep in mind. Indeed, life is so very complex and so very rich that memory finds itself altogether inefficient in preserving the great store of practical information that a record of its experience could teach. The individual cannot and need not remember its content; he needs only to remember the existence of its linguistic record and where to find it. This latter memory becomes one great symbol for information that it has taken a lifetime to collect, and this symbol is a substitute for, and serves the purpose of, the original memory of it all. But this is the least of the functions that language can and has performed in furnishing a means for guiding the individual's conduct. With it the individual transcends the narrow limits of his own past, and his own consciousness. The past of the race, so far as recorded in language, becomes the common heritage of every individual. Compared with this vast store of practical information that the race has contributed for the use of future generations that of the individual stands as of little or no consequence. Language has made this accessible. The significance of the memory image in its old form as an immediate means to adaptation has dwindled almost to zero.

In giving to language this function we have included already the results of other factors that were mentioned for consideration. The experience of the race is more than the collective *immediate* experience of many individuals. But for the moment let me regard that experience as strictly and purely em-

pirical. It is the recorded gross observation of man, left in its first form without being reduced to general rules and laws of nature. It is data for science, but not science. It is memory data cast into a linguistic record, not the result of a rational treatment. Viewed from this standpoint the insufficiency of memory as a means to adaptation and for guiding conduct, as compared with science, becomes at once quite apparent. That insufficiency is the result of the short comings of our sensory data. Our memory alone cannot transcend the material furnished by the senses. And nature has given us a set of senses which (although taken together, they are the best in the animal series), are yet a poor makeshift as a direct means to a perfect adaptation. Among the innumerable forces in nature and the forms in which they express themselves there are very few indeed that it is given to us to know directly through our sensory equipment. A small scale of sounds we can hear in a range of vibration frequencies in nature that we have not even the means to imagine. A certain intensity we can sense, but below the threshold lies a world of sounds for which we can only infer an existence. At other points in the scale we pick out small groups of vibrations that we sense as heat or light, but the vast creation of forms of forces in matter and ether we may long to know, and we may think about, but we cannot know directly. Small distances and large objects we can see, but we add telescopes and microscopes to our eyes and with them we wonder at the mysteries of structural nature that we cannot puzzle out. Yet that knowledge, which lies beyond the immediate reach of our senses, we must have, for it has the highest sort of survival value. We must know nature fully. This knowledge, which we might have, had we the proper sense organs to obtain it, we are slowly acquiring in another way. We are obtaining it through a laborious task of inference and reasoning, the results of which as expressed in the sciences mark progress towards better means to better adaptation. The method of adaptation is here far removed from that which consists only of direct sensory data and memory of that data or the linguistic record of that memory. And to the extent that this is the case the memory image in its old form, however recorded, has lost most of its significance, its survival value. In language is recorded not only the empirical experience of the race, but also the history of its sciences. Against this background of knowledge that of the individual is lost sight of almost entirely. In fact, few individuals attain the mental scope to make a practical use of more than a very small portion of the race's contribution to science.

But science has another use than has so far been implied.

We have considered science as a means to adaptation and for guiding conduct only in so far as it supplements the insufficient sensory data. The memory of our immediate experience can never get beyond what the senses furnish, and if this is inadequate, memory must be at least to the same extent inadequate. Science has extended sensory knowledge. But this extension merely by added observations is science's smallest function. The memory image of the individual's immediate experience has been called the means to a remote adaptation, because upon the basis of it the individual could adjust his reaction beforehand in the expectation of meeting the same situation again. But science has supplied a means to remote adaptation in a far larger sense than this. The empirical observations are reduced to general rules and laws of nature. In some places at least we know nature to such an extent that we know what she will do before she does it. We need not wait for that experience; we can tell what it will be beforehand and can adjust our procedure accordingly. The experience that may injure or cost the life of the individual before he can know how to adjust his behavior can be avoided through the knowledge that science affords. Science looks to the future far beyond the limits of the individual's memory of his own experience. It looks into the past also far beyond the reach of either the individual's or the race's memory.

In this consideration of the larger relations between memory and reason in science we have lost sight for the moment of what rôle they play in the everyday life of the individual when he is neither attempting to be scientific nor particularly conscious of guiding his conduct according to the dictates of the memory of his previous experience. Life is wider than merely its utilitarian aspects; and conduct consists of more than adaptation of reaction with reference to survival. In the fuller and richer scope of consciousness, what places do memory and reason hold? The main fact in answer to this question may be stated here at once. In the vast majority of instances man's actions are a matter of habit, in which neither reason nor conscious memory enter, or follow from accepted authority, in which case reason and conscious memory are equally absent. Man is regarded as the only rational being, but it is only in very rare moments that he is consciously rational. It has taken nature a long time to produce this supreme function in the animal organism, and it requires the results of the last stage in the development of structural complexity. It is too precious a function to be employed in the usual and ordinary activities of life. The guidance of these is handed over to other methods.

If to this extent we must depreciate the function of reason in everyday life, we must to an equal, if not greater extent, de-

preciate the function of mere memory. It has been said that the function of forgetting is as important as is that of remembering. This touches one aspect of a fact to which we may turn our attention for a moment. It is not so much in how much we remember, but in how we select in our remembering that the mind becomes efficient and makes progress. If we remembered with equal tenacity all the experiences of our life, that condition would be as fatal as that of the absolute absence of memory. Attention was called before to the insufficiency of our sensory data as a direct means to adaptation. In the contemplation of this fact we should not forget that our salvation lies in part in forgetting practically all of the limitless mass of data that is constantly being presented to consciousness. Of the many thousands of impressions that reach most of our sense organs every hour, it is only the very exceptional one that is remembered the next hour; it is only the very exceptional one that rises above the threshold of consciousness sufficiently to become the object of attention even for an instant. Of the many incidents that we attend to during a day, of the thoughts we think, and the emotions we experience, it is a very rare one that we honor with a second thought. If we ask why this should be so, the answer is at once that these things have no interest, no significance, no value. It is, indeed, one of the marks of superiority that the mind *can* disregard them. Attention to them would mean so much energy wasted, so much distraction from the more serious and important affairs of life, a break in the continuity of a consistent purpose that runs, or ought to run, through all our conscious living. But what is significant in this for our present consideration is the fact that mere memory takes here a subordinate place. It is not the indiscriminate retention and recall of all our experiences but the proper selection of data for memory that is significant; a factor of superior importance comes in that makes the proper use of the memory faculty, and it is through this proper use alone that memory becomes of value to life. What this more important factor is is difficult to describe. We cannot say that we, as a rule, make a rational selection of what is to be remembered and what is to be disregarded. In the majority of instances we certainly do not reason it out. It is rather the whole life of the individual that enters to decide.[1]

This view depreciates mere memory as the factor that determines what is to become a more or less permanent part of our mental life and what in our experience is to be excluded.

[1] This aspect of the matter and those that I touch upon below have been luminously treated by Stratton: Experimental Psychology and Culture, N. Y., 1903, pp. 187-195.

There is one other matter to be noted in which memory alone does not play so great a rôle as might seem, or as is usually supposed. With those experiences that we do wish to retain and attempt to recall the naïve assumption is that, after the original experiences are once past, it is memory alone that decides what and how we shall recall them. But the phenomena of memory illusion and our own introspection shows that this is not so. In the attentive recall of our experiences we read into our memories things that we do not really possess as memories; we remember things as we think they ought to have been. In this our knowledge of the order of nature, or other logical motive, takes the place of memory, and reconstructs our experience where memory finds itself deficient. Through a long schooling with nature we have acquired an insight into her ways, and when we are called upon to say how such and such an event took place it is this insight as much or more than it is memory that is determinative for us. We have many memory illusions, but we never remember a thing as having occurred contrary to what we think is a law of nature. "After all," says Stratton, "and especially when we try to interrelate events lying in the more distant past, our main dependence is upon our knowledge of how things *ought* to go together, rather than upon simple sensations or emotions or upon the elements of distinctness. We learn some of the more elementary laws of nature and, guided by them, set up certain mnemonic landmarks; and then, with these, we connect our subordinate memories, knowing, as we do, what their causal relation was, and what order they *must* have had." In this connection the influence of conscious memory in acquiring that insight is of course not to be overlooked. But when it is once acquired it has a higher function than conscious memory can exercise. In this reconstruction of the past by insight, and not by conscious recall, the process *may be* that of conscious inference, but as a rule it is not. Insight like perception is the result of a long training of the individual and of the race, in which nearly all of the steps that it was necessary to take to attain it have dropped out. In it the voice of our past speaks in an effective but unobtrusive tone. We can recall few of the events of our childhood, and of the things that occurred in that long period of learning that has made the adult mind. But the past that is forgotten is not necessarily lost. In some form, by some method, we continue to profit by that experience, and it is more important to have had a good past than to be able to recall it.

The matter of insight into the laws of nature and the construction of the past and the future through it rather than directly through conscious memory is a process of slow acquisition that belongs to the race much more than it does to the

individual. And in this connection it is interesting to raise the question as to the probable place of conscious memory among mental functions in the future of the race. In regard to this topic I can do no better than to quote another passage from Stratton. "With us at present," he says, "memory is the necessary means of rising superior to memory. Our insight is not as yet sufficient to enable us to detect in our immediate and present perceptions a wide range of unperceived facts. So that we must depend upon memory to furnish the large store of experience with which reason works. It is consequently through our power of recollection that we attain that preliminary familiarity with nature and its laws by which we are able later to turn upon memory, correct it, and even shake ourselves free from its dominion. The course of development, therefore, it seems probable, is from an initial state, in animals and children, which is without consciousness of the past, through a period of memory and recollection, and then onward towards a condition of even more perfect consciousness of the past than memory gives; but by insight and not by mechanical retention. Memory thus stands between these extremes as a happy transition expedient, an easy makeshift, mercifully given us during the days of our ignorance, so that we may have the world before us without the need of understanding it. Some such thought seems intended by Beatrice when she says to Dante that the angels have no need of memory, because there is no interruption of their vision; they see all things constantly reflected in the divine countenance."

# ON CONDITIONS AFFECTING THE MAXIMAL RATE OF VOLUNTARY EXTENSOR AND FLEXOR MOVEMENTS OF THE RIGHT ARM.

By ROBERT HARVEY GAULT.

## INTRODUCTORY.

Experiments on the maximal rate of extensor and flexor movements of both arms in various classes of persons have been for some time in progress at the Laboratory of Psychology of the University of Pennsylvania. The results of this investigation, when published, will contribute material bearing on differences in maximal rate of movement for race, sex, for the right and left arms, and for extension and flexion. In the course of this enquiry several factors have appeared to exert a determining influence upon the rate of these movements. Not only are such general factors as practice and fatigue effective in modifying the rate but the position of the subject with reference to the instruments, his mental attitude toward the investigation, the stimulation which he receives from the directions of the experimenter, the direction of his attention to the various parts of the movement; all these and other factors from time to time are observed to affect the rate of movement of some or all subjects.

This monograph proposes to subject to the analysis of the experimental method certain of these factors that determine the maximal rate of voluntary movement. Its scope is restricted to an examination of certain conditions affecting the initial and preparatory stage of movements of maximal rate, *i. e.*, those in which the volitional element is at a maximum. For the purposes of this investigation I have employed extensor and flexor movements of the right arm.

It would be imposible within the limits of an inquiry of moderate proportions even to begin an examination of all the influences that probably have a share in the production of these movements. The factors with which I shall be concerned in this monograph are:

1. Various extents of running start;
2. The mean variations of the average rate of movement;
3. The back pressure or the backward start, *i. e.*, a pressure of variable amount exerted by the hand and arm against the post from which the movement is started;
4. The duration of the back pressure or backward start;

5. Various conditions such as practice, fatigue and the differential rate of extensor and flexor movements which have arisen incidentally in the course of the investigation.

The back pressure exerted barely enters the field of the subject's consciousness. The image of the end of voluntary movements is so vivid that the initial and preliminary stages cannot be successful rivals excepting by special effort on the part of the observer, and in this case the movement usually suffers either in speed or in accuracy or in both. But though these stages of movement are so dimly perceived the adjustments which take place within them are of no less interest to the psychologist who keeps a place in his scheme of psychology for reflex and automatic movements. It is doubtless due to the position which they occupy in the field of consciousness that they have received so little attention at the hands of psychologists and physiologists.

The studies of the initial stage of movement considered as the time of reaction, it is true, would fill volumes. But these studies usually do not include an examination of the relation between the reaction time and the movement which follows. On the other hand, the literature bearing upon the problem of this investigation is meagre. Camerer, in 1866 (2),[1] described as the "form" of a voluntary movement the characteristic successive variations in the rate of a horizontal movement of the arm from the beginning to the end of the course. The movement is at first slow, then increases, and finally decreases in rapidity. Camerer, however, did not employ movements of maximal rate. His results therefore do not bear directly upon the problem of the present investigation. They bring to light, nevertheless, at least one item of interest in this connection. Although the form of movement is approximately the same for extensor and flexor movements, the highest velocity in slow extensor movements is reached a little later in the course than the highest velocity in flexor movements. In other words, the running start, during which the arm is approaching its maximal velocity, is longer in extensor than in flexor movements. But nothing in the literature justifies the conclusion that the same difference would be found in movements of maximal rate.

The investigations of Loeb and Koranyi in 1890 (6) on horizontal movements of an arm lightly loaded, and those by Binet and Courtier in 1893 (1) on rapid writing movements appear to confirm Camerer's conclusion in a general way but add nothing of importance bearing on the problem of this investigation.

In 1892 Fullerton and Cattell (4:114 f.) made a brief report

[1] See References at the end of this paper.

of a few experiments on the maximal rate of movement. Their general procedure as well as the details of method are very nearly those which I have followed in my investigation. They employed similar extensor and flexor movements of the right and left arms, a course of the same extent, *i. e.*, 50 cm., and placed the subject in approximately the same position with reference to the apparatus which differed only in detail from mine. They report that the average time of a movement of the arm over 50 cm. in 10 separate series of 10 experiments each with, however, one subject only, varies from $96\sigma$ to $138\sigma$. They believe this variation points to differences in the condition of the subject. In the same subject flexion of the right arm is found to be slower by $7\sigma$ than extension of the same arm. This contradicts the report of Charles Fere, in 1889 (3), to the effect that flexion of the upper limbs is not only more energetic but of greater velocity than extension. Fere admits, however, the possibility of occasional exceptions. As to the variations in the velocity of a single continuous movement of maximal rate from the beginning to the end of this course, Fullerton and Cattell are of the opinion that the rate is high at the beginning and diminishes toward the end of the movement.

The second problem of this investigation, to find the effect of the preliminary backward start or back pressure upon the subsequent movement, was first studied by Woodworth, in 1901(9), who examined the extent of the upward movement of the clenched fist in its relation to a downward blow upon a table. He found that a forcible blow is not always accompanied by a great extent of preliminary upward movement. Although no constant relation appears between the force of the blow and the extent of the upward movement, by selecting a large number of the more forcible blows, he found that the average of the upward movements preceding them is greater than that of the upward movements accompanied by less forcible downward blows.

In an investigation of "antagonistic reactions" in 1903, W. G. Smith (8) discovered that about one-third of his subjects showed an unmistakable inclination to increase the downward pressure upon the key just before reacting. It is Smith's opinion that from the psychophysical point of view antagonistic reaction is due to the idea dominant in consciousness before the reaction being that of holding the finger pressed upon the key. When the signal for reaction comes it is quite conceivable that the first effect of the shock would be a more decided realization of the motor idea already in consciousness: *i. e.*, a more decided push upon the key. No attempt was made by Smith to find what relation, if any, exists between the antagonistic reaction and the subsequent movement.

In a study of reaction time and an outward rotatory movement of the arm, T. V. Moore (7:55 ff.), in 1904, found no constant relation between reaction time and movement time, and no evidence of an antagonistic reaction. It is probable, however, that he overlooked the latter for he says (7:13) that while waiting for the signal to react his subjects often "unwittingly" broke and re-made contact between the rotating arm of his instrument and the contact post.

The most recent study of the preliminary stage of movement is that of Judd, McAllister and Steele (5), published early in 1905. This is an investigation of the phenomena described by W. G. Smith. The experiments show that preparation for the final reaction movement by a gradual movement in the direction of reaction is, with most subjects, less favorable for a speedy reaction than the gradual antagonistic movement, (5:153.) On the other hand sudden antagonistic reactions are cases of excessive effort which is not always so applied as to be favorable to the speed of reaction. (5:171.) The result seems to justify their conclusion that a reaction is not a simple movement. Between the warning signal and the stimulus there is always a complex process of adjustment. (5:163.) The comparison of the effects of the gradual and the sudden antagonistic movement upon the final reaction appears to be the first contribution bearing upon one problem of my investigation, *i. e.*, the relation between the duration of the antagonistic phenomenon and the subsequent movements.

## Apparatus and Method.

This investigation permits of the treatment of my experiments in two main divisions: first, those experiments which had to do with the effect of various extents of running start upon the rate of movement; and next in order, those dealing with the back pressure. My apparatus and methods differed somewhat in these two groups of experiments. In both groups, however, the time of the movement was recorded by the Hipp chronoscope controlled to a constant and variable error of less than one sigma by means of the Cattell drop screen. During the course of the investigation from the middle of October, 1904, to the end of February, 1905, 93 control series of 10 trials each were made. The variations of the averages of these 10 consecutive trials from the standard control time of the Cattell screen vary between $0.4\sigma$ and $1.3\sigma$, the average constant error being $0.5\sigma$. The mean variations range from $0.27\sigma$ to $0.89\sigma$, the average mean variation being $0.56\sigma$.

For the first group of experiments, namely, those dealing with the effect of the running start upon the rate of movement, we required a device to mark the beginning and the end of the

recorded part of the movement. For this purpose I used two instruments, one of which served as the starting-post of the movement and will be called the "starting contact," the other usually served as the finishing-post and will be called the "finish-contact."

In my experiments *without a running start*, the starting contact (See Fig. 1, A.) is adjusted to close the chronoscopic cir-

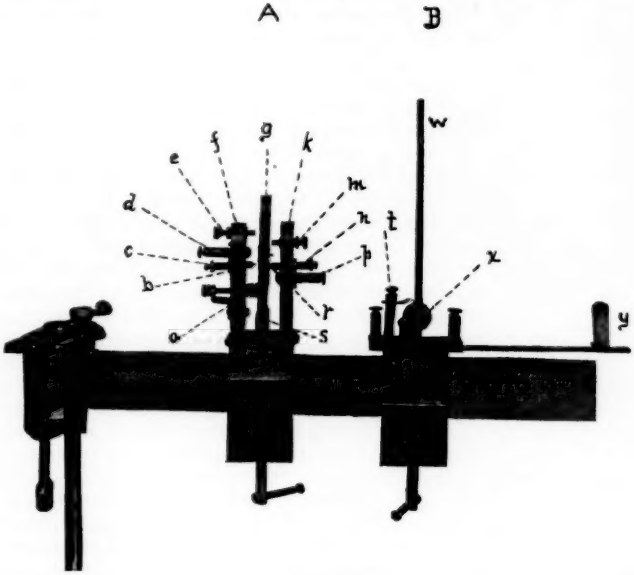


FIG. I.

A STARTING CONTACT.

a coiled spring.
b plunger pin of contact set screw.
c contact set screw.
d binding post for contact set screw.
e set screw acting as back-stop for starting-post.
f left upright of frame.
g starting-post movable about axis "*s*."
k right upright of frame.
m set screw acting as back-stop for starting-post.
n contact set screw.
p binding post for contact set screw.
r binding post for contact plate on starting-post.

B FINISH CONTACT.

t binding post.
w hickory stick, "finish post."
x ebonite contact disc.
y spring catch.

cuit at the beginning of a movement. When in use the instrument is fastened to the edge of a laboratory table or, as in my experiments, to a horizontal wooden bar, by means of a clamp which forms the lower part of the contact piece. The part of the instrument which extends above the level of the bar when it is clamped in position, as in Fig. 1, A, consists essentially of two wooden uprights, "*f*" and "*k*," fastened securely to the base. Between these and projecting above them is a light bar of ebonite "*g*," 9 x 2 x 0.5 cm. which I shall call the "starting-post." This post is movable through an arc limited by the distance, 2.5 cm., between the two uprights. By means of a set screw projecting from each upright, *e. g.*, "*e*" and "*n*" for movements to the right of the starting-post, the movement of the post may be restricted to any desired extent. In my experiments the set screws were adjusted to permit of a movement of the starting-post of 1 mm. at the level of the lower set screw "*n*," at which point the electrical contact was made. In practice the subject placed the first finger of the right hand, the back of the hand being uppermost, against the movable starting-post and held it in position against the set screw "*e*" of the upright on the left of the instrument. The hand was held in this position while the subject awaited the signal to make the required movement. The pressing back of the starting-post against the set screw is accomplished against the resistance of the adjustable spring "*a*." When the hand is removed from the starting-post the spring shoves the post vigorously forward over the small distance permitted by the set screw "*n*." This set screw carries at its inner extremity a plunger contact pin of platinum. The plunger pin "*b*" of the opposite set screw "*c*" is clearly visible in the illustration. That of the set screw "*n*" is thrust back into the body of the set screw by the pressure of the starting-post. On the face of the starting-post just opposite the contact set screw is a thin plate of platinum connected with the binding post "*r*." The circuit is closed by the plunger pin in the set screw "*n*" impinging upon the surface of this platinum plate.

This "starting-contact" is a double one. The upper set screw "*m*" of the upright on the right of the instrument, "*k*," may be brought into function with the set screw "*c*" and plunger pin "*b*" of the binding post "*d*." The starting-post carries on its left surface a second platinum plate which is insulated from that described above on the right surface, and is connected with a binding post on the reverse edge of the starting-post as seen in Fig. 1 A. The spring "*d*" may be adjusted to exert a pull to the left upon the "starting-post" instead of pushing it to the right as in the arrangement described above. Thus by changing the upper set screws and by adjusting the

spring, this "starting-contact" may be used to record a movement either toward the right from the starting-post or toward the left. In my experiments the apparatus was adjusted only as described in the first instance: to record movements toward the right. When movements in flexion were to be recorded, which naturally required a movement toward the left from the starting point, the subject stood back of the instrument, as represented in Fig. 1 A, thus placing the starting-post on his right and enabling him to move from it toward the left.

The finish contact (Fig. 1 B), employed to break the circuit at the end of such part of the movement as it was desired to record, consists essentially of a slender hickory stick "*w*," 15 cm. long, rotating about a horizontal axis which carries, in addition to the stick, which we shall call the "finish-post," a contact disc of ebonite "*x*," which may be so adjusted with reference to a platinum brush connected with the binding post "*t*," that the slightest movement of the finish-post from any given position will break, or, if desired, close an electric circuit. When used as a finish-post the contacts are so adjusted that the circuit is closed when the post is vertical. A slight movement of the post to the right will then break the circuit. As the post is often violently knocked down the instrument is provided with a spring catch "*y*," to prevent any possible rebound from closing the circuit after it has once been broken. The instrument shown in the illustration is more complicated than was necessary for my experiments. The horizontal axis is provided with a second disc similar to "*x*" so that two circuits may be controlled by the same movement of the post.

These two instruments were clamped to a horizontal bar at such distance apart that the stretch between the "starting-post" and the "finish-post" exactly equalled 50 cm. plus the thickness of the subject's right index finger. In extensor movements, the subject faced the instruments as represented in Fig. 1, with the centre of his body about ¼ of the length of the entire stretch from the starting-post to the finish-post. In flexor movements the subject faced the back of the instruments, as shown in Fig. 1, placing himself about ¾ of the distance of the entire stretch from the starting-post to the finish-post. When these instruments are clamped to the edge of a table around which it is not convenient for the subject to move, the finish-post shown in Fig. 1 B, is used to the right of the starting-post and another finish-post is clamped at the desired distance to the left, the latter instrument being appropriately modified for use in movements toward the left.

This set of three contact pieces was designed by Professor Witmer for the Laboratory of Psychology as an improvement on the rate of movement apparatus described by Fullerton and

Cattell. The Cattell instrument is cumbersome and difficult to set up outside of the laboratory. The Witmer set is easily transported, may be set up wherever a table can be found, and when once adjusted permits of movements of the right and left hand in both flexion and extension from nearly the same position.

In experiments *with a running start*, the starting-post described above and the finish-post were clamped upon the bar at a distance equal to the movement desired to be measured plus the running start intended to be allowed, which varied in my experiments from 0.25 cm. to 7.5 cm. At a distance from the starting-post equal to the running start, another instrument similar to that described as the finish-post was clamped to the horizontal bar. But in this case, the contacts on the horizontal axis were so adjusted that the circuit was open when the post was vertical, but closed upon the smallest movement of the post to the right. The circuit maker of the starting-post "*g*" Fig. 1, was thrown out of function and the subject started his movement from the starting-post as described above. At the signal he moved his hand as rapidly as possible to the finish-post, striking on the way the post inserted at the beginning of the stretch over which the time of movement is to be recorded.

For the second group of experiments, namely, those dealing with the effect of the preliminary backward start or "back pressure" upon the rate of movement, the apparatus, consisting of several instruments, was adapted to perform the following functions: (1) to close the chronoscope circuit at the beginning of the movement; (2) to break the chronoscope circuit at the end of the part of a movement intended to be recorded; (3) to measure the amount of "back pressure" at the start of this recorded movement in terms of grams or some other convenient unit; and (4) to measure the duration of this pressure. One of these instruments is illustrated in Fig. 2 and appears schematically also in the diagram, Fig. 3. The frame-work of the instrument (see Fig. 2), which is made of hardwood, consists of a baseboard "*A B*," 1 M. x 18 cm. x 2.5 cm., at one end of which is erected an upright support "*E*," 40 x 18 x 1.5 cm. From the upper extremity of this upright projects a horizontal arm "*D*," 18 x 3 x 2.5 cm., from which hangs a lever "*C*," 50 x 2.7 x 1 cm., suspended from a pivot pin "*h*" about which as an axis the lever is free to move in a vertical plane. This movement, however, is limited by a slot in the baseboard, 17 cm. long, through which the lever projects to a distance of 10 cm. below. This lever carries a make and break contact operated by a thin spring tongue of sheet copper 9 x 1 cm. against which the subject presses the index finger of the hand at the point "*g*." The upper end of this tongue is held tight

against the lever by the screw of the binding post "*k*." The excursion of the lower end of this spring tongue is limited by a yoke, "*f*," of sheet copper which is fastened to the lever by the binding post "*p*." The tongue of copper is adjusted to bear against the yoke so as to close the chronoscope circuit passing through the binding posts "*k*" and "*p*." When the subject presses against the starting lever the spring tongue is pushed back through a distance of 1 mm. against the surface of the lever.

For my experiments on the duration of the back pressure I needed also to record the time during which the subject exerted pressure upon the starting lever. For this purpose I employed another circuit passing to the spring tongue through the binding post "*k*" and from the spring tongue to the binding post

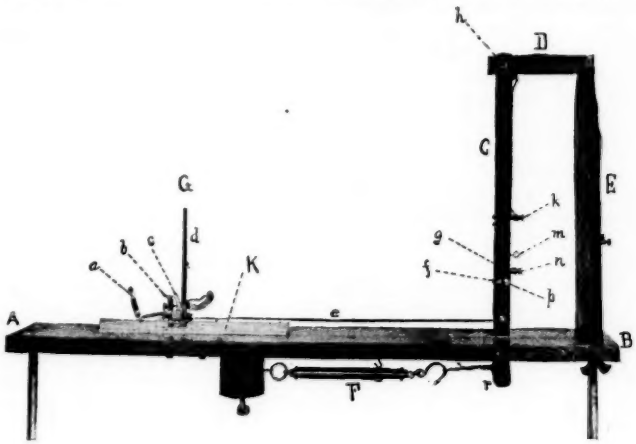


FIG. II.

A B baseboard.
C lever.
D horizontal arm supporting the lever C.
E upright support for arm D.
F spring balance.
G finish contact.
K track supporting finish contact G.

a spring catch for post d.
b binding post.
c brush.
d finish post.
e cord connecting lever C with finish contact G.
f yoke on starting contact.
g spring tongue of starting contact.
h pivot pin of lever C.
k binding post on spring tongue of starting contact.
m attachment for cord to marker W. Fig. 3.
n binding post for electro-magnetic marker circuit.
p binding post on contact yoke.

"*n*," which passes through the lever and offers a bearing on the surface next to the spring tongue.

The finish post "*d*" of the finish contact "*G*," a slender stick 15 cm. long, operates as does the "finish-post" "*w*" on the finish contact illustrated in Fig. 1, B. This post "*d*" is fixed to an axis bearing a contact disc rotating about the axis when the post is moved to or from its vertical position. This disc is so adjusted that a slight movement of the post "*d*" to the left from the vertical position breaks an electrical contact with the brush "*c*" projecting from the binding post "*b*." A spring catch "*a*" prevents the rebound of the post "*d*" after it has been forcibly knocked down. When the starting and finish contacts just described are placed in circuit with the chronoscope we have a rate of movement apparatus analogous to that illustrated in Fig. 1.

The measurement of the amount of backward start or back pressure is accomplished through the following special features of the instrument. The lever "*C*" extends through a slot in the baseboard and is attached below at "*r*" to a spring balance

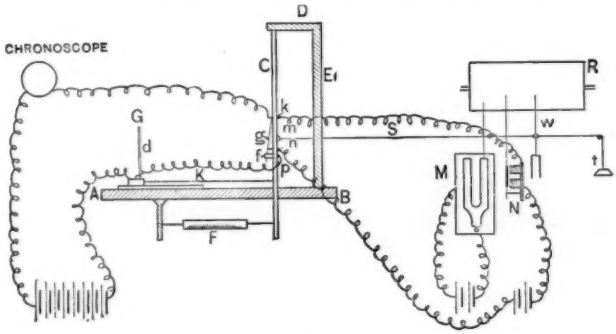


FIG. III.

A B baseboard.
C lever.
D horizontal arm supporting the lever C.
E upright support for arm D.
F spring balance.
G finish contact.
N electro-magnetic marker.
R kymograph.

d finish post.
f yoke on starting contact.
g spring tongue of starting contact.
k binding post on spring tongue of starting contact.
m attachment for cord to marker "w."
n binding post for electro-magnetic marker circuit.
p binding post on contact yoke.
s cord connecting lever C with marker "w."
t counterweight.
w kymograph marker.

"*F*" which is graduated in grams. From the scale of this balance, an observer can read in grams the amount of pressure exerted by the subject toward the right upon the lever "*C*." From this reading, the pressure exerted by the subject may be calculated by using the formula for a lever of the third order, the ratio of the arms being 1:1.5 when the subject's finger is held at "*g*." In practice, however, I did not obtain the "back pressure" readings directly from the spring balance "*F*." A light cord, "*s*" (see diagram Fig. 3) fastened to the lever "*C*" at "*m*" and passing through a slot in the perpendicular "*E*," was run through a pulley and held taut by means of a light weight. To this cord was attached a marker "*w*," by means of which any displacement of the lever "*C*" due to pressure exerted upon it toward the right, could be graphically recorded upon the smoked surface of the kymograph "*R*." By a simple computation, which will be set forth in the treatment of results, the operator can determine at the close of each series just how much "back pressure" is represented by each displacement of the marker recorded upon the kymograph sheet.

To keep the distance constant between the starting lever and the "finish-post" "*d*" despite variations in the displacement of the lever, a cord "*e*" ties the finish contact "*G*" securely to the lever "*C*." When the lever "*C*" is displaced toward the right, the "finish-post" is drawn after it upon the track "*K*" upon which it lightly rests. To reset the apparatus for a new experiment, the post "*d*" must not only be replaced in vertical position after each movement, but the finish contact "*G*" to which it is attached must be slid back upon the track "*K*" until the cord "*e*" is taut. A light brass rod was employed for this purpose at first, but was supplanted by the cord, owing to the friction of the rod on its bearings.

The duration of the back pressure was measured in hundredths of a second from the direct tracing of a 100 V. Koenig fork. The higher rate of speed at which the kymograph was necessarily run to obtain this tracing drew out the curve of back pressure made by the marker "*w*" to such an extent as to obscure the point at which the curve fell away from the horizontal, when back pressure on the lever "*C*" was released. The point at which the curve rose from the horizontal when the lever "*C*" was displaced remained readily distinguishable. I therefore used this point of the curve traced by the marker "*w*" to indicate the moment at which the back pressure began. To indicate the moment at which the back pressure ceased to act upon the lever "*C*" I employed the marker "*N*," Fig. 3. This marker made its tracing upon the kymograph in the same straight line with the points of marker "*w*" and the marker of

the tuning fork. The marker "*w*" was in a circuit connected with the binding posts "*k*" and "*n*," Figs. 2 and 3. The binding post "*k*" is connected with the metal tongue against which the subject presses his finger. The post "*n*" is a thumb screw with a platinized point projecting through the lever "*C*." When the subject presses his hand upon the metal tongue at the point "*g*," and thereby breaks the chronoscope circuit at the yoke "*f*," he presses the copper tongue against the platinized point of the thumb screw contact "*n*" thus closing the circuit through the marker "*N*." The release of back pressure upon the lever "*C*" is followed by (1) a movement of the metal tongue to the left thus breaking the circuit of the marker, and (2) a simultaneous movement in the same direction of the lever "*C*" communicated through the cord "*s*" to marker "*w*." I therefore expressed the duration of back pressure in terms of the number of vibrations of the tuning fork "*M*" occurring between the initial displacement from the horizontal of the line traced by "*w*" and the break in the line traced by "*N*" upon the release of the pressure of the spring tongue of the contact.

The same general procedure was employed throughout the experiments. At a given signal the subject placed the index finger of his right hand, back uppermost, against the starting-post, and, holding himself in readiness to make a movement of the hand at maximal rate toward the finish-post, awaited the signal to move. When this signal was given he knocked over the finish-post with as rapid a movement of the hand as possible. The subject is made to understand, however, that he may delay the start of the movement until he finds himself ready for a maximal effort. Details of the method will be introduced in connection with the treatment of results. It is obvious that the only way to guarantee a perfectly horizontal movement from the starting to the finish-post is to employ some device for guiding the hand. This would probably introduce so much constraint on the part of the subject that it was thought better to allow a perfectly free movement and to neglect the slight errors in time arising from this small variation in the extent or direction of movement.

## Results.

The results of this investigation fall into two main divisions corresponding to the two groups of experiments mentioned above. They show, first, the effect of the running starts of various extent, and second, the effect of preparatory back pressure and its duration, upon the maximal rate of movement.

Experiments upon seven subjects furnished the data for the first group of results. One of these subjects, always designated in the tables by B., is a young woman, an undergraduate

student in biology. The others are men, and all are graduate students in psychology with the exception of T., who is an instructor in psychology. Only the last named subject had any experience in experiments on the rate of movement before this investigation was begun.

In the experiments which constitute the first group of my results, the subjects invariably made a series of twenty successive extensor movements, followed after a brief period of rest by a series of the same number of flexor movements. By alternating the series of extensor and flexor movements the effects of practice and fatigue may be expected to be distributed evenly over the results of both flexion and extension, thus rendering possible a satisfactory comparison of extensor and flexor times. The interval between the movements in a series was no greater than was necessary to allow the operator at the chronoscope to record the reading. During this interval the subject reset the instrument for the next experiment by raising the "finish-post" or "finish-posts," to the vertical position; in the second main group of experiments he also shoved the finish contact (see Fig. 2), back on its track "*K*" until the cord "*e*" was taut. Not more than ten series of twenty experiments each, in all 200 movements, divided equally between flexion and extension, were made by the same subject in one day. Usually a day's work was restricted to eight series. The average and mean variation of each series of extensor and flexor movements were computed and these formed the basis for the collation of the results presented in Table I.

The set of experiments which came first in chronological order was composed of a series without a running start. Three subjects, S., H., and B., made 800 extensor movements each, or forty series, and the same number of flexor movements. Two subjects, T. and M., who entered somewhat later into the investigation, made 160 extensor movements or eight series of experiments, and the same number of flexor movements.

The values which are reported in the lines opposite the letter which stands for each of these five subjects (see Table I) are the grand averages of the averages of the separate series. I give in this table in vertical column below the caption "Subj." the letters which represent the names of the five subjects. In the second column under the caption "Movt." I indicate by "Ex." or "Flex." whether the line contains values of extensor or flexor movements. In the third column headed "o." I give for each subject the grand average of the extensor and flexor experiments comprising what I have called the first set, namely, those made without running start. At the bottom of the table I give the combined averages of the extensor and flexor movements of the five subjects, and also, for a special

purpose which I shall have occasion to refer to shortly, the averages of the three subjects only, S., H., and B.

By referring to the table it will be seen that the extensor movement is performed by my five subjects, on the average, in 116.1$\sigma$ and the flexor movement in 102.4$\sigma$, a decisive advantage in favor of the flexor movement of 13.7$\sigma$. An inspection of the individual values will show that the five subjects differ greatly among themselves in average rate of speed. The most rapid are S. and T. who are tall and possessed of long arms. Next in order

TABLE I.

| | | Time. | | | | | Mean Variation. | | | | | Excess of Ex. over Flex. | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Subj. | Mov't | 0 | 0.5 | 1 | 3 | 7.5 | 0 | 0.5 | 1 | 3 | 7.5 | 0 | 0.5 | 1 | 3 | 7.5 |
| S. | Ex. | 83.2 | 71.5 | 69.8 | 81.6 | 67.1 | 5.0 | 9.3 | 3.9 | 2.9 | 2.6 | 4.9 | 5.2 | 9.7 | 10.9 | 9.3 |
| | Flex. | 78.3 | 63.8 | 60.1 | 70.7 | 57.8 | 5.6 | 2.3 | 3.6 | 2.9 | 1.7 | | | | | |
| H. | Ex. | 122.6 | 89.7 | 83.9 | 80.7 | 91.4 | 8.5 | 5.2 | 4.1 | 5.0 | 4.7 | 18.2 | -9.8 | 4.7 | -5.4 | 2.2 |
| | Flex. | 104.4 | 99.5 | 79.2 | 86.1 | 89.2 | 10.1 | 4.7 | 5.2 | 4.2 | 4.9 | | | | | |
| B. | Ex. | 155.7 | 112.2 | 109.3 | 99.1 | 99.7 | 12.1 | 4.9 | 5.0 | 3.6 | 5.3 | 25.2 | 21.5 | 15.6 | 18.0 | 14.2 |
| | Flex. | 130.5 | 90.7 | 93.7 | 81.1 | 85.5 | 12.0 | 4.4 | 4.9 | 3.2 | 5.6 | | | | | |
| T. | Ex. | 94.7 | | 81.2 | 73.8 | 72.5 | 4.8 | | 2.8 | 3.1 | 2.3 | 8.5 | | 8.8 | 13.7 | 10.9 |
| | Flex. | 86.2 | | 70.5 | 60.1 | 61.7 | 7.1 | | 3.4 | 2.3 | 1.1 | | | | | |
| M. | Ex. | 124.3 | 118.3 | 98.7 | 94.2 | 114.6 | 14.5 | 3.4 | 3.1 | 2.2 | 5.3 | 11.5 | 9.2 | 11.5 | 19.1 | 24.9 |
| | Flex. | 112.8 | 98.4 | 87.2 | 75.2 | 89.7 | 18.3 | 4.8 | 4.6 | 3.4 | 5.6 | | | | | |
| Total Avs. | Ex. | 116.1 | 97.9 | 88.6 | 85.8 | 89.0 | 8.9 | 4.2 | 3.8 | 3.4 | 4.0 | 13.7 | 9.8 | 10.5 | 11.2 | 12.2 |
| | Flex. | 102.4 | 88.1 | 78.1 | 74.6 | 76.8 | 10.6 | 4.0 | 4.3 | 3.2 | 3.8 | | | | | |
| Avs. of S. H. B. | Ex. | 120.5 | 91.1 | 87.6 | 87.1 | 86.1 | 8.6 | 4.5 | 4.3 | 3.8 | 4.2 | 16.1 | 5.6 | 10.0 | 7.8 | 8.6 |
| | Flex. | 104.4 | 84.6 | 77.6 | 79.3 | 77.5 | 9.2 | 3.8 | 4.5 | 3.4 | 4.0 | | | | | |

Average times in sigma and average mean variations of extensor and flexor movements over 50 cm. without a running start, and with four extents of running start. For the number of experiments of which these are the collated averages see text, page 369. The table gives also the excess of extensor over flexor values.

are H. and M. who are both men with short arms, while B., who has the longest times, is a woman of somewhat less than medium stature. These differences between the subjects apply with respect to both extension and flexion, that is, a subject who is relatively more rapid in extension is also more rapid in flexion. The differences in favor of flexion which may be found in the column headed "0." in the third group of columns, under the caption "Excess of Ext. over Flex." vary from a minimum of 4.9$\sigma$ with the most rapid mover to 25.2$\sigma$ with the slowest mover.

The average mean variations of the series of experiments in this set is found in the first column headed "0." of the group

of columns under the heading "Mean Variation." The average mean variation of the five subjects is 8.9$\sigma$ for extension and 10.6$\sigma$ for flexion. Thus the mean variation for flexion is greater than the mean variation for extension despite the fact that the average value of an extensor movement is greater than the average value of a flexor movement. This difference between the average mean variation holds true for all the subjects with the exception of B. whose average mean variations favor flexion by 0.1$\sigma$. This subject has the longest average values for the movements. It would appear from this result that flexion may be more rapidly executed than extension but a maximal rate is less constantly maintained.

The second set of experiments in chronogical order are made with a start of 3 cm., the third with a start of 7.5 cm., the fourth with a start of 0.5 cm., and the fifth with a start of 1 cm. Each subject, excepting T. and M., made 520 extensor and the same number of flexor movements or 26 series of each movement. T. and M. made but 8 series. From these results I obtained the average values and the mean variations set down in the columns headed by appropriate captions.

The order in which these sets of experiments were made was adapted to enable us to distinguish the effect of practice from that of the running start. The average extensor time without a running start was found to be 116.1$\sigma$. Although the subjects had been allowed three series of extensor and flexor movements each, by way of practice, it is to be presumed that the effect of practice would continue to make itself felt throughout this first set of experiments, and, indeed, to some extent, through the other sets. The average extensor time for all subjects in the second set with a running start of 3 cm. is 85.8$\sigma$, a difference of 30.3$\sigma$ apparently in favor of the running start. This difference, however, may be due in part or in toto to the effect of practice. When we consider next the average extensor time with a running start of 7.5 cm., that is 89$\sigma$, we find a difference of 27$\sigma$ in favor of the longer running start in comparison with the average time without a running start, but a difference of 3.2$\sigma$ in favor of the shorter running start, 3 cm., in comparison with the longer running start, 7.5 cm. We must conclude from these values that the difference between the values with 3 cm. and 7.5 cm. start is due not to practice but to the running start. Practice and a running start have produced a low value for 3 cm., but the shorter running start is favored over the longer running start despite the greater practice had in the latter set of experiments. The propriety of this analysis receives further justification on considering the values of the fourth and fifth sets, namely, 97.9$\sigma$ with a running start of 0.5 cm., and 88.6$\sigma$ with a running start of 1 cm. Of all the experi-

ments with running start those with 3 cm. were made with the least practice. Despite this fact the average value is the smallest. The average value with a running start of 0.5 cm. is the largest of those with the running start. It would appear from these results that as we increase the running start from 0 to 3 cm. we reduce the rate of movement, and this reduction is of such an amount as to obscure the expected effects of practice. As we pass from a running start of 3 cm. to the longer running start of 7.5 cm. we appear to increase the time of movement, and this despite the fact that practice would lead us to look for a reduction in the average time of movement.

That a running start should reduce the average rate of movement is to be expected. To enter into the explanation of the cause of this reduction is hardly necessary. It is a question of some uncertainty whether the greatest amount of reduction due to the running start is to be expected at the very beginning of the running start and therefore to show most with running starts of very small extent or whether the decrease is due to the getting up of speed which may be supposed to increase proportionately with the extent of the start. My results for extensor movements show the greatest reduction with the smallest running start in comparison with the movement without a running start. I am not permitted to draw any conclusions from this fact at this place owing to the experiments with running starts having been performed late in the investigation and it being impossible to distinguish the effect of this running start from that of practice.

If the reduction in rate of movement is proportional to the extent of the running start we may be surprised at the increased time of movement with a start of 7.5 cm. over the time of those having a 3 cm. start. The difference between these values, as we have said above, represents a true difference between the running starts. The cause for this difference is to be sought in an examination of the results of the individual subjects.

The course of the results for flexor movements is practically the same as that for extensors. The average time of all the subjects without a running start in flexor movements is 102.4$\sigma$. This time reduces to 88.1$\sigma$ with a running start of 0.5 cm., to 78.1$\sigma$ with a running start of 1 cm., to 74.6$\sigma$ with a running start of 3 cm., and increases to 76.8$\sigma$ with a running start of 7.5 cm.

The results of the individual subjects do not in every case conform to the course of those of the averages of all the subjects. It is not to be expected that practice would operate on all the subjects alike, and we may therefore expect when we compare 3 cm. with 0.5 cm. that we will not find in every case the same decrease as we proceed to the longer start. Every

subject shows a considerable decrease in average time with 0.5 cm. from the values obtained without a running start, and a smaller decrease when the running start is increased to 1 cm. This is true of both extensor and flexor movements. But when we increase the start to 3 cm. subject S. shows a considerable increase in both extension ($69.8\sigma$ to $81.6\sigma$) and in flexion ($60.1\sigma$ to $70.7\sigma$) and H. shows an increase in flexion ($79.2\sigma$ to $86.1\sigma$). The other seven values conform to the average result. When we compare the individual results with the running start of 3 cm. and 7.5 cm., we find a decrease in the time of movement for S. in extension ($81.6\sigma$ to $67.1\sigma$) and flexion ($70.7\sigma$ to $57.8\sigma$), and for T. in extension ($73.8\sigma$ to $72.5\sigma$). The other seven values for the longer running start conform to the average result, though the values with 3 cm. and 7.5 cm. for the flexor movements of subject T. are practically the same. S. and T. have a long reach, H., B., and M. have a short reach. Indeed the practical conditions of the experiments showed a stretch of 50 cm. plus a running start of 7.5 cm., to be too long for these three subjects and for this reason I reduced my extent of recorded movement to 35 cm. in a subsequent group of experiments. I believe, therefore, that the increased average time with a running start of 7.5 cm. is due to the diminished rates at the end of the movements owing to the short reach of three of my subjects. So far as my results show at this point the effect of a running start is always to reduce the time of movement, and up to and including a start of 3 cm. it is approximately proportional to the length of start.

At the conclusion of these five sets of experiments. a sixth set *without running start* was made upon my three most practiced subjects S., H., and B. The results of this set gave me standard values for the purpose of comparing effects of running starts after practice has been eliminated. Sixty experiments or three series were made on each subject with both extensor and flexor movements. For the purpose of comparing the averages of these subjects without a running start after practice with their averages with a running start I show in Table 2 the results of the sixth set of experiments with these subjects in the column under the caption "With practice." The other values in this table I have taken directly from Table 1. From Table 2 it will be seen that the average extensor time for the three subjects, without a running start and without practice, is $120.5\sigma$; after practice it is $101.5\sigma$. The average time of these subjects in Ex. with a running start of 0.5 cm, is $91.1\sigma$, with a running start of 1 cm. it is $87.6\sigma$, with a running start of 3 cm. it is $87.1\sigma$, and with a running start of 7.5 cm. it is $86.1\sigma$. We see from these results that the great difference between the movement without a running start and with the smallest run-

TABLE II.

| | | Time. | | | | | | Excess of Ex. over Flex. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Without Practice. | With Practice. | | | | | Without Practice. | With Practice. | | | | |
| Subj. | Mov't | 0 | 0 | 0.5 | 1 | 3 | 7.5 | 0 | 0 | 0.5 | 1 | 3 | 7.5 |
| S. | Ex. | 83.2 | 77.0 | 71.5 | 69.8 | 81.6 | 67.1 | 4.9 | 0.4 | 7.7 | 9.7 | 10.9 | 9.3 |
| | Flex. | 78.3 | 76.6 | 63.8 | 60.1 | 70.7 | 57.8 | | | | | | |
| H. | Ex. | 122.6 | 96.8 | 89.7 | 83.9 | 80.7 | 91.4 | 18.2 | 3.4 | -9.8 | 4.7 | -5.4 | 2.2 |
| | Flex. | 104.4 | 93.4 | 99.5 | 79.2 | 86.1 | 89.2 | | | | | | |
| B. | Ex. | 155.7 | 130.8 | 112.2 | 109.3 | 99.1 | 99.7 | 25.2 | 28.5 | 21.5 | 15.6 | 18.0 | 14.2 |
| | Flex. | 130.5 | 102.3 | 90.7 | 93.7 | 81.1 | 85.5 | | | | | | |
| Av. | Ex. | 120.5 | 101.5 | 91.1 | 87.6 | 87.1 | 86.1 | 16.1 | 10.8 | 6.5 | 10.0 | 7.8 | 8.6 |
| | Flex. | 104.4 | 90.7 | 84.6 | 77.6 | 79.3 | 77.5 | | | | | | |

Average times in sigma and excess of extensor over flexor values. Taken directly from Table I. excepting the 0 column "with practice" which are the results of the sixth set of experiments and show the effect of practice upon the time of movement without a running start and the approximation (Subjects S. and H.) of extensor and flexor times as a result of practice.

ning start is not entirely due to practice. The difference is 10.4$\sigma$, a further decrease due to doubling the running start is 3.5$\sigma$, and a tripling of the running start decreases the average time only 0.5$\sigma$. With flexor movements this decisive reduction in the time with the smallest running start is not so apparent. The average time without a running start and without practice is 104.4$\sigma$, and with practice it is 90.7$\sigma$. This time is reduced when a running start of 0.5 cm. is given to 84.6$\sigma$; a reduction of 6.1$\sigma$. When the running start is doubled to 1 cm. the time is reduced to 77.6$\sigma$ a reduction of 7$\sigma$. When the running start is increased to 3 cm. the time increases to 79.3$\sigma$. Conformity with the rest of our results leads us to expect a decrease in time where we find this increase. This discrepancy is really due to the diminished value, 77.6$\sigma$, when the running start is 1 cm. The reduction in the average is contributed by one subject, H., who has the value 79.2$\sigma$ as the average time with 1 cm. This is far below what we should expect from his other results. I can in no way account for the short times which he gives for the running start of 1 cm. If we exclude this result from the average, thereby increasing the time of flexion with 1 cm., we find that the course of the reduction in movement with flexion corresponds to that with extension, namely, the greatest reduction in time is found with the smallest running start of 0.5 cm., a reduction next in amount with 1 cm. and a further small re-

duction with 3 cm. It would appear, therefore, from these results that the greatest reduction in time is made by the smallest running starts 0.5 cm. and 1 cm. Beyond this extent of running start the times appear to reduce slowly. It is probably fair to conclude, when different subjects traverse a stretch of 50 cm. at a maximal rate of speed, that relatively the greatest loss of time occurs within the first half centimeter of the stretch, certainly within the first centimeter. It is likely that a very considerable part of the recorded time is involved in the actual start of the movement. The elimination of the actual start disposes of a great part of the difference between the rates of movements for different subjects. It should also be considered in this connection that the experiments designated as "*without running start*" were actually made with a start of 1 mm., the distance through which the starting post moved before electrical contact was made and the chronoscope began to record. As we perhaps have a reason to expect from the analysis of the production of voluntary movements the start of the movement is a critical point, not only during the course of preparation preceding the actual start, but for the fraction of a second after the movement is actually begun.

The values in Table 2 under the caption "Excess of Ex. over Flex." show the effect of practice upon the difference between the time of extensor and flexor movements. This effect is especially noticeable in the two columns under "0," the first of which contains values based upon the results of the first set of experiments without a running start and without previous practice, and the second contains values based upon the results of the sixth set which was made without a running start but with the advantage of practice in all the preceding sets of experiments. An inspection of the total averages of these two columns shows that the excess of extensor movement time over flexor movement time has been reduced from 16.1$\sigma$ to 10.8$\sigma$. In the case of S. the reduction is from 4.9$\sigma$ to 0.4$\sigma$; in the case of H. from 18.2$\sigma$ to 3.4$\sigma$. As to B., however, the result is quite different: the excess has increased from 25.2$\sigma$ to 28.5$\sigma$. In other words for the two subjects S. and H., extensor and flexor movement times have approximated each other. This approximation is effected by the greater decrease in extensor than in flexor movement times. This result is shown in Table III in which I have isolated the extensor and flexor movement times for S., H., and B., with no running start, without practice and with practice respectively, and have shown in the column on the right the decrease in extensor and flexor times respectively for each subject. Extensor times for S. decreased 6.25$\sigma$ and flexor times only 1.7$\sigma$; for H. extensor times decreased 25.80 and flexor times only 11.0$\sigma$. In the case of B., on the

TABLE III.

| Subject. | Movement. | Without Practice. σ | With Practice. σ | Decrease. |
|---|---|---|---|---|
| S. | Ex. | 83.2 | 77.0 | 6.25 |
| | Flex. | 78.3 | 76.6 | 1.7 |
| H. | Ex. | 122.6 | 96.8 | 25.8 |
| | Flex. | 104.4 | 93.4 | 11.0 |
| B. | Ex. | 155.7 | 130.8 | 24.9 |
| | Flex. | 130.5 | 102.3 | 28.2 |

other hand, the greater decrease was in flexor times; 28.2σ against 24.9σ. This probably indicates that in the case of subjects S. and H. the extensor muscles of the arm were under less perfect control than the flexors when the experiments of this investigation began.

Granting that this be true, we must not suppose, in analogy with so many processes of learning, that there is, at the outset, a relative imperfection in the connection of the nervous tracts leading from the cortical centres to the root cells of the motor nerves for the extensor muscles of the arm. This would be the argument in a study of reaction-times but in the present case it may or may not be true. It is with the time required for movement and not with the motor impulse from the cortex that we are concerned. The core of the present problem is in the education or training of the muscle itself and probably also in the training of afferent tracts from the muscle to the cord and the corresponding efferent tracts. For if we estimate the rate of the nervous impulse between the extensor of the arm and the brachial region in the cord at 27 M. per sec. we find that in 40σ, which is about one-half of the duration of an extensor movement over 50 cm., the nervous impulse can travel over a little more than one meter. But this is a greater distance than that between the extensor of the arm and the brachial region. It would seem, therefore, that in the course of a movement occupying 85σ or 90σ, and probably in movements of shorter duration, there is time for a sensory impulse from the muscle to reach the cord, and for a motor impulse to return in time to re-enforce the primary impulse which initiated the movement. The more quickly after the beginning of the movement this re-enforcement occurs, if it occur at all, the greater will be the acceleration of the movement within the 50 cm. course and consequently the shorter the time required to make the desired movement. Camerer (2 : 25) showed that in movements not of the maximal rate, the instant of greatest velocity in the extensor movement occurs nearer the end of the course than in

the flexor movement. It is possible that the same would be found true of movements of intended maximal rate and 50 cm. in extent. It might even be found that the moment of maximal velocity is subsequent to the moment at which the 50 cm. limit is passed. In this case, obviously, any means that would hasten the instant of maximal velocity so as to bring it within the time required in covering the course of 50 cm., or whatever it may be, would have the effect of increasing the rate of movement over the whole course. Whether any such process has occurred in these experiments, there is no means of knowing since my apparatus is not adapted for obtaining any information on this point. It is necessary to have the variations in rate from the beginning to the end of each separate movement throughout a long course of training before any conclusion can be offered in this matter.

I have already called attention to the fact that the average mean variation of all subjects for extensor movements is less than for flexor movements, despite the fact that the extensor movements are of longer duration than the flexor movements. The course of the mean variation compared with the average times presents little of interest, the mean variation varying with the values themselves. It is perhaps worth calling attention to the small average mean variation, $1.1\sigma$ of Subject T. in flexion with 7.5 cm. start (see Table I). Another subject, S., has a variation with the same start of but $1.7\sigma$. These variations represent a remarkable constancy in the rate of movement. The relatively large variations of the time values of B. and M. for 7.5 cm. start indicate perhaps a greater unsteadiness owing to the short reach of each subject. The total averages of the mean variations follow the same general course as the total averages of the time values. There is a sharp decrease in the averages of the 0.5 cm. column, as compared with the averages of the 0 column; from $8.9\sigma$ to $4.2\sigma$ and from $10.6\sigma$ to $4\sigma$ respectively, with differences of $4.7\sigma$ and $6.6\sigma$ for extension and flexion respectively. There is a slight increase of the average for flexion with a start of 1 cm. over that with a start of 0.5 cm.; $4.3\sigma$ compared with $4\sigma$. With this exception there is an almost uniform decrease in the average values until the 7.5 cm. start is reached where a slight rise occurs in conformity with the general result of the time of movement. It is perhaps worthy of notice that this slight irregularity in the course of the average variations is due to the large mean variations of B. in both extensor and flexor movements and of H. in flexor movements. Consequently the same disturbance occurs in the averages of the three most practiced subjects, S., H., and B., given at the foot of Table I.

During an extended course of experiments it is to be ex-

pected that diurnal variations in the time of movement, resulting from the inconstant physical condition of the subjects will occur.

TABLE IV.

| Subj. | Movt. | 0 | 0.5 | 1 | 3 | 7.5 |
|---|---|---|---|---|---|---|
| S | Ex. | 3.5 | 4.3 | 4.7 | 2.7 | 1.0 |
| | Flex. | 3.4 | 5.0 | 5.8 | 3.5 | 1.1 |
| H | Fx. | 5.1 | 4.3 | 4.1 | 4.9 | 5.0 |
| | Flex. | 5.5 | 5.2 | 4.5 | 2.9 | 4.2 |
| B | Ex. | 3.7 | 3.1 | 5.6 | 2.5 | 2.8 |
| | Flex. | 5.1 | 4.8 | 4.1 | 3.1 | 3.0 |
| Total Av. Daily Var. | Ex. | 4.1 | 3.9 | 4.8 | 3.4 | 2.9 |
| | Flex. | 4.6 | 5.0 | 4.8 | 3.2 | 2.8 |

In these experiments, however, the variations of this kind were slight and there is no evidence that they have affected the general results. The values represented in Table IV are those of the mean variations of the daily mean variations of the time of movement from their average. They pertain to three subjects, S., H. and B. The results obtained from subjects T. and M. are excluded from this table because their experiments under each condition of running start, were made in one day. The values in the column headed "0." are the mean variations of ten daily variations from their average. They are the results of all experiments made with no running start, within a period of four weeks at the beginning of the year 1904-1905. The values represented in the remaining columns are in each case the mean variations from the average of six mean daily variations and the experiments on which each column is based covered a period of two weeks. Below, and opposite the caption "Total Av. daily Var." are the averages of the diurnal variations of all subjects.

The result is very satisfactory. The average daily variation of extensor movements ranges from $2.9\sigma$ to $4.8\sigma$, and of flexor movements from $2.8\sigma$ to $5.0\sigma$. Experience has led us to expect just such variations as these from fairly practiced subjects even in the course of a single series of experiments. The statement that diurnal variations do not affect the final results of these experiments seems therefore to be justified. Notwithstanding this I decided to conduct a series of tests of the maximal rate of movement under all the conditions of running start which had been employed in the preceding experiments, but from which the possibility of diurnal variations in rate should be eliminated,

and from the beginning to the end of which each subject should be in an approximately uniform condition as regards practice. It was necessary accordingly to obtain from each subject in a single experimental period a record of his best effort under all the conditions of running start. To make this practicable without fatiguing the subjects I employed a series of only ten flexor and ten extensor movements. The course over which the time of movement was to be recorded was reduced from 50 cm. to 35 cm. in order to bring it easily within the reach of my shortest armed subjects. The successive series of experiments were made in the order of the increasing running start. The first series was with no running start, the second with a start of 0.25 cm., which was not employed in the former experiments, the third with 0.5 cm. start, and the fourth, fifth and sixth series were made with a start of 1 cm., 3 cm., and 7.5 cm. respectively. Five subjects took part in these experiments, of whom S., H., and B. were practiced in the experiments which have just been reported. G. and E. were unpracticed subjects. The results of these experiments are tabulated in Table V. Each value in this table represents an average of only ten experiments; a single series. In the third column from the left of the table under the caption "0." are the averages of the ten extensor and ten flexor movements without a running start made by each of the five subjects who are designated by initial in the first column. The averages of the movement times with running starts in succeeding series are tabulated in the same manner under the appropriate captions and the corresponding mean variations are set down in the middle group of columns from left to right under the caption "Mean Variations." On the right are the differences between the times of flexor and extensor movements. At the foot of the tables are the total averages of the averages of extensor and flexor movements of all subjects and also the separate total averages of the values for three practiced subjects, S., H., and B.

By referring to the total averages in the tables it will be seen that the average of all subjects' average extensor movement time is $78.4\sigma$, while that of the flexor movement time is $69\sigma$. The averages of the extensor and flexor values in the column under the caption 0.25 cm. are respectively $77.5\sigma$ and $68.2\sigma$; a reduction of only $0.9\sigma$ and $0.8\sigma$ respectively from the total averages of the first column. The averages for extensor and flexor movements respectively in the columns headed by 0.5 cm., are $77\sigma$ and $66.8\sigma$; a further reduction of $0.5\sigma$ and $1.4\sigma$. From this point the decrease of the total averages is more rapid; from $77\sigma$ to $72.7\sigma$, and from $66.8\sigma$ to $58.8\sigma$ for extension and flexion respectively, with a start of 1 cm. With a start of 3 cm. the averages descend to $67.2\sigma$ and $55.3\sigma$; an ad-

TABLE V.

| Subj. | Mov't | Time. | | | | | | Mean Variation. | | | | | | Excess of Ex. over Flex. | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 0 | 0.25 | 0.5 | 1 | 3 | 7.5 | 0 | 0.25 | 0.5 | 1 | 3 | 7.5 | 0 | 0.25 | 0.5 | 1 | 3 | 7.5 |
| S. | Ex. | 62.4 | 61.8 | 63.2 | 57.6 | 53.8 | 51.3 | 3.7 | 3.9 | 1.8 | 2.4 | 3.6 | 2.2 | 2.5 | 4.6 | 9.2 | 8.2 | 7.8 | 9.2 |
| | Flex. | 59.9 | 57.2 | 54.0 | 49.4 | 46.0 | 42.1 | 3.1 | 2.9 | 1.4 | 1.5 | 1.0 | 1.2 | | | | | | |
| H. | Ex. | 89.1 | 89.0 | 88.1 | 81.8 | 76.0 | 72.4 | 4.1 | 3.9 | 3.3 | 2.9 | 2.2 | 2.7 | 8.8 | 7.6 | 8.9 | 11.7 | 6.7 | 8.1 |
| | Flex. | 80.3 | 81.4 | 79.2 | 70.1 | 69.3 | 64.3 | 5.6 | 5.9 | 5.2 | 3.0 | 2.3 | 2.4 | | | | | | |
| B. | Ex. | 98.5 | 97.9 | 96.1 | 86.7 | 80.4 | 72.4 | 5.5 | 3.2 | 2.4 | 2.2 | 2.1 | 3.1 | 21.5 | 19.7 | 20.5 | 20.0 | 13.1 | 12.2 |
| | Flex. | 77.0 | 76.2 | 75.6 | 66.7 | 59.7 | 50.8 | 2.1 | 3.4 | 4.1 | 5.5 | 1.7 | 2.2 | | | | | | |
| G. | Ex. | 67.4 | 64.9 | 65.0 | 64.5 | 59.7 | 50.8 | 2.1 | 3.4 | 4.1 | 5.5 | 1.7 | 2.2 | 2.0 | 1.1 | 1.9 | 14.7 | 14.6 | 9.4 |
| | Flex. | 65.4 | 63.8 | 63.1 | 49.8 | 45.1 | 41.4 | 2.7 | 2.6 | 2.3 | 1.4 | 2.1 | 2.2 | | | | | | |
| E. | Ex. | 74.8 | 74.0 | 73.8 | 73.0 | 66.1 | 63.2 | 4.8 | 4.7 | 3.9 | 1.8 | 1.7 | 3.8 | 7.1 | 13.1 | 11.7 | 14.6 | 10.8 | 7.7 |
| | Flex. | 67.7 | 62.7 | 62.1 | 58.4 | 56.3 | 55.5 | 6.9 | 5.6 | 3.0 | 1.9 | 2.7 | 1.9 | | | | | | |
| Total Avs. | Ex. | 78.4 | 77.5 | 77.0 | 72.7 | 67.2 | 62.0 | 4.0 | 3.8 | 3.1 | 2.9 | 2.2 | 2.8 | 9.8 | 9.2 | 10.4 | 13.8 | 10.6 | 9.3 |
| | Flex. | 69.0 | 68.2 | 66.8 | 58.8 | 55.3 | 52.3 | 4.2 | 4.2 | 3.3 | 2.5 | 2.0 | 1.9 | | | | | | |
| Avs. of S. H. B. | Ex. | 83.3 | 82.9 | 82.4 | 75.3 | 70.1 | 65.3 | 4.4 | 3.6 | 2.5 | 2.5 | 2.6 | 2.6 | 10.9 | 10.6 | 12.8 | 13.3 | 9.2 | 9.8 |
| | Flex. | 72.4 | 71.6 | 69.6 | 63.1 | 58.3 | 54.9 | 3.8 | 4.3 | 3.8 | 3.0 | 1.8 | 1.8 | | | | | | |

Average times in sigma and average mean variations of extensor and flexor movements over 35 cm. without a running start, and five extents of running start. Each value is the average of ten. The table gives also the excess of extensor over flexor values.

ditional decrease of $5.5\sigma$ and $3.5\sigma$ respectively for extensor and flexor movements. Finally, with a start of 7.5 cm. there is a further decrease of $5.2\sigma$ and $3\sigma$ for extensor and flexor movements respectively, *i. e.*, from $67.2\sigma$ to $62\sigma$, and from $55.3\sigma$ to $52.3\sigma$. These differences indicate a very slight decrease in the time of movement with a start of even 0.5 cm. and a rapid and almost uniform decrease up to and including the longest running start, 7.5 cm. An inspection of the individual values in the table shows no series of results the course of which does not run parallel with that of the final averages.

The total averages of the mean variations follow approximately the same course as the averages of the times movement. There is, however, one discordant result, namely, the average mean variation of extensor movements, $2.8\sigma$, in the column under the caption 7.5 cm. This result is $0.6\sigma$ higher than the total average of mean variations in extension in the column headed 3 cm. This disturbance of the uniformity of the final result is introduced by one of the unpracticed subjects, E., whose mean variation in extensor movements with a start of 7.5 cm. is $3.8\sigma$ against $1.7\sigma$ with a start of 3 cm. The average mean variations of the three most practiced subjects, S., H., and B., are still less in accord with the total averages of the time of movement, due chiefly to subject B.'s large variations

in flexor movements with a start of 0.5 cm. and 1 cm., and to the want of uniformity in S.'s mean variations in both extensor and flexor movements. The total average movement times of these three subjects, while higher than the total average of all subjects, conforms to the general result.

The experiments of the second main division of this investigation were undertaken for the purpose of finding the effect of back pressure and the time during which it is exerted, upon the time of movement over a course of 50 cm. with no running start.

For the purpose of the first set of these experiments, namely, to find the effect of "back pressure" alone upon the time of movement, the instrument illustrated in Figs. 3 and 4 was brought into use but only the marker "*w*," Fig. 3, was employed on the kymograph. The data for the following results were obtained from eight subjects, He., Ma., E., St., W., J., Mc., and V. He., E., St., and V., were graduate students in psychology, Ma., was a visitor at the laboratory, and the others, W., J., and Mc., were graduate students in chemistry and biology. All of these subjects were unpracticed excepting E., who had had practice in the one series of experiments with a 35 cm. course under the various conditions of running start. It is desirable to employ practiced subjects in these experiments provided they are ignorant of the purpose of the investigation. All of my practiced subjects, however, had knowledge of the object of the experiments, and it was feared that this would affect the results which might be obtained from them in this part of the investigation. They were therefore excluded, and the data which I shall present in the following pages is collected only from unpracticed subjects.

Three or four preliminary trials were made with each subject in order to make him familiar with the apparatus and the signals. The amount of back pressure that each subject would probably exert was determined in these preliminary trials, and the tension of the spring in the balance "*F*" (see Fig. 2) was adjusted accordingly. It was apparent, even from these preliminary trials, that there would be large individual variations in the amount of back pressure. If the tension of the spring in the balance "*F*" were constant there would, in some cases, be a large, and in other cases a slight displacement of the lever "*C*." But in order to eliminate as far as possible, difference in the extent of backward start as a factor in the results, the tension of the spring at the beginning of each series was adapted as nearly as could be to the probable amount of back pressure that would be exerted by each subject as indicated in the preliminary trials. Some subjects therefore began the push-back against a heavier initial dead weight than others. This

inequality in conditions, it seems to me, is of little if any consequence in these experiments inasmuch as the tension of the spring was constant for each subject. The tension of the spring was usually set at 100 g. though in some cases it was as low as 0., and in others at intermediate points, determined in each case by preliminary experiment.

After a series of ten extensor and ten flexor movements the operator has the chronoscope readings and also the tracings of the marker "*w*," Fig. 3, upon the smoked surface of the kymograph. The latter are measured and the value of each excursion of the marker "*w*" is computed in grams. A single example will illustrate the method. Suppose the tension of the spring in the balance "*F*" is such that the indicator points to 100 g. Now if the lever "*C*" is pushed back until the indicator on the balance points to 200 g., the marker "*w*" has made an excursion of 8 mm. on the kymograph. The subject in such a case has overcome the initial 100 g., and has added another 100 g. to the reading on the balance. But he has been pushing at "*G*" against a lever of the third order in which the length of the whole lever (50 cm.) is one and one-half times that of the long arm (33.3 cm.). Hence in the above case the amount of back pressure is equal to 1.5 (100 g.+100 g.) = 300 g. That is with the tension of the spring at 100 g. an excursion of 8 mm. on the kymograph represents a pressure of 300 g. All computations of back pressure were made in a similar manner.

The ten chronoscope records and the ten records of back pressure were averaged to obtain the values which are set down in Table VI. In this table in the column under the caption "Subj." are the initials of the eight subjects who aided in these experiments, and on the right of this the two columns under the caption "Pressure" contain the average back pressure in

TABLE VI.

| | Pressure. | | Time. | |
|---|---|---|---|---|
| Subject. | Ex. | Flex. | Ex. | Flex. |
| He. | 416 | 650 | 92.7 | 87.3 |
| J. | 350 | 475 | 127.3 | 110.1 |
| Mi. | 280 | 435 | 120.2 | 100.3 |
| W. | 240 | 384 | 115.2 | 101.4 |
| Ma. | 233 | 420 | 106.7 | 80.5 |
| V. | 220 | 285 | 144.7 | 120.1 |
| E. | 210 | 260 | 153.3 | 115.3 |
| St. | 206 | 363 | 110.6 | 90.4 |
| Av. | 269.3 | 409.06 | 121.3 | 100.7 |

grams exerted by each subject in extensor and flexor movements respectively. The last two columns on the right contain the averages of the times of these movements.

In every case a longer time is required for extensor than for flexor movements and the back pressure in extensor movements is always less than that in flexor movements. In the case of He., for example, the time for extension is 92.7$\sigma$, and the back pressure is 416 g. The time for flexion, on the other hand, is 87.3$\sigma$, and the back pressure is 650 g. Taking these eight subjects together the average time of extensor movements, shown at the foot of Table VI, is 121.3$\sigma$; of flexor movements, 100.7$\sigma$. The corresponding averages of back pressure are 269.3 g. and 409.06 g. respectively. That is, while the average time of flexion is 20.6$\sigma$ shorter than that of extension, the average back pressure in flexion is 139.76 g. higher than in extension. A study of the individual results showed, however, that a single very rapid movement is not invariably preceded by a high degree of back pressure. Moreover, a very rapid movement is sometimes preceded by a less intense pressure than a slower movement. The averages in Table VI show further that a subject whose time is short does not invariably exert a high degree of back pressure. Ma., *e. g.*, whose extension time is next to the shortest gives a low back pressure in extension; only 233 g. and though his flexion time is shortest on the list, namely 80.5$\sigma$, there are three (He., J., and Mi.), whose back pressure in flexor movements is greater than his.

If the amount of initial back pressure were an important factor in determining the time of the subsequent movement we should find that a large difference between the times of flexor and extensor movements occurs together with a large difference between the back pressure exerted in preparation for the same movements. In an attempt to find such a correspondence in the case of the eight subjects of these experiments I have taken the differences between the back pressure values and the time values in Table VI and from these differences I have constructed Table VII. The pressure differences for each subject in the "Pressure" column are arranged in order from the largest to the smallest. In the "Time" column on the

TABLE VII.

| Subject. | Pressure. | Time. | Subject. | Pressure. | Time. |
|---|---|---|---|---|---|
| 1 He. | 234. g. | 5.4 (8) | 5 W. | 144. | 13.8 (7) |
| 2 Ma. | 187. | 26.2 (2) | 6 J. | 125. | 17.2 (6) |
| 3 S. | 157. | 20.2 (4) | 7 V. | 65. | 24.6 (3) |
| 4 Mi. | 155. | 19.9 (5) | 8 E. | 50. | 38. (1) |

right are the differences between each subject's times for flexion and extension. The figures in parentheses on the right of this column designate the place which each value would occupy if placed in order from the largest to the smallest. The subject "*He.*" who has the largest difference in pressure, 234.5 g., has the smallest difference in time, 5.4$\sigma$. The subject *E.* who has the smallest difference in pressure, 50 g., has the largest difference in time, 38$\sigma$. Another subject, V., who has the next smallest difference in pressure, 65 g., has a large difference in time, 24.6$\sigma$. If we exclude these three subjects from consideration, we find with the other five subjects that the excess of pressure with the flexor movement is correlated with a corresponding reduction in the time of flexor movements, although the order of subjects *W.* and *J.* for time of movement is the reverse of that for pressure, the difference in neither case, however, being large. The evidence presented in this table, not very strong to be sure, appears to warrant the conclusion that on the average the more rapid movements are preceded by a higher degree of back pressure and the slower movements by lower pressure. My results show unequivocally that greater back pressure occurs in preparation for flexor movements. This means, of course, that there is a more intense antagonistic contraction of the extensor than the flexor muscles. Perhaps in the motor area of the cortex dispersion of excitation occurs more readily in the direction of the centre of the extensor than of the flexor muscles. This would not necessarily contradict the facts as they appear in spastic paralysis, in which disorder there is a continuous strong contracture of the flexor muscles. In these cases the cortical excitation has had time to overcome the greater resistance toward the flexor centres—granting that there is a greater resistance in this direction—whereas in these experiments the cortical excitation and its dispersion cover a period of only several hundredths of a second at most, and the resistance toward the flexor centres can probably be only partially overcome in so brief a time.

The results of the experiments which had to do with the final purpose of this investigation, namely, to find the effect of the time during which back pressure is exerted upon the time of movement, are necessarily based upon meagre data owing to the tedious nature of the experiments. In addition to the marker "*w*," Fig. 3, which traces the pressure curve upon the kymograph, we require the electro-magnetic marker "*N*" which is actuated by a current passing through the posts "*k*" and "*n*," and marks on the kymograph the point in the curve traced by the marker "*w*" at which back pressure ceases and the movement is begun. The tuning fork "*M*" traces the time line by which the duration of back pressure is determined.

The kymograph was therefore necessarily run at such high speed that though it carried fully three meters of paper it was a matter of great difficulty to get more than 8 or 10 records upon a single sheet.

The five subjects of these experiments were graduate students in chemistry and biology. All were unpracticed excepting Wi., who made but one series of ten flexor and ten extensor movements in the preceding set which had to do with the effect of back pressure upon the time of movement, and all were ignorant of the object of the experiments.

The results of these experiments are embodied in Table VIII, in which are shown the time of movement, the back pressure measured in grams, and the duration of back pressure measured in fractions of a second for each subject in extension and flexion. Each value represents an average of four or five.

In the case of Wi. the average time of the extensor movements, 106.3$\sigma$, is shorter than that of the flexor movements, 149.6$\sigma$, and the shorter time of back pressure, 0.08 sec., precedes the shorter movement; extension 106.3$\sigma$. In three cases out of the five, O. S., and Mc., in flexion, and Ha. in extension, the longer average duration of back pressure occurs with the shorter time of movement.

TABLE VIII.

| | Extension. | | | Flexion. | | |
|---|---|---|---|---|---|---|
| Subj. | Time. | Back Pressure. | Time of Pressure. | Time. | Back Pressure. | Time of Pressure. |
| Wi. | 106.3 | 168.75 | 0.08 | 149.6 | 187.5 | 0.1 |
| O. S. | 97.0 | 375.0 | 0.44 | 74.0 | 431.25 | 0.78 |
| Mc. | 89.0 | 800.0 | 0.53 | 74.0 | 950.0 | 1.06 |
| P. | 82.7 | 60.0 | 0.275 | 68.3 | 483.0 | 0.23 |
| Ha. | 96.97 | 65.7 | 0.626 | 100.6 | 46.65 | 0.52 |
| Av. | 94.39 | 293.89 | 0.39 | 93.3 | 417.68 | 0.54 |

That is with subject O. S., the duration of back pressure in the shorter, or flexor movements, is 0.78 sec. as compared with 0.44 sec. in the longer or extensor movements. With Mc. it is 1.06 sec. with the shorter and 0.53 sec. in the longer time of movement, and with Ha. it is 0.626 sec. in the shorter and 0.52 sec. in the longer time of movement. On the other hand, in the results obtained from Wi. and from P., the longer duration of back pressure occurs with the longer time of movement. In the first case it is 0.1 sec. with the longer movement compared with 0.08 sec. with the shorter movement, and in the second, 0.275 sec. with the longer and 0.23 sec. with the shorter. The

averages of the values in Table VIII show that on the whole the longer duration of back pressure (0.54 sec.) occurs with the briefer or flexor movement ($93.3\sigma$) but the evidence of the individual results is not decisive. It is worthy of notice, however, that the results obtained from each of these subjects, excepting Wi., confirms the results of the preceding set of experiments inasmuch as they show that the greater amount of back pressure occurs on the side of the more rapid or flexor movements. For the one exception to this general result I am not able to account.

It is well known that in the education of a boxer the first precaution is to train him out of the habit of "telegraphing," as it is called; or drawing the arm backward in preparation for a stroke. In the light of the experiments discussed in this paper, and of those reported by Woodworth on "The Voluntary Control of the Force of Movement," (9), it seems to me that the boxer's backward start before dealing a blow may, on the whole, aid him in making a quick and forcible stroke. If this is true the only justification for training him out of this behavior lies in the fact that the backward start tells the opponent when and where the expected blow will come. Since the boxer may be trained out of this antagonistic action it seemed probable that my most practiced subjects may, in the course of practice, have trained themselves out of the back pressure preparation for arm movements.

Accordingly the matter was put to trial with S. and T. both of whom knew the purpose of the test. The former acted as my subject in all the experiments of the first division of this investigation; namely, those to determine the effect of the running start upon the time of movement. While T. had considerably less practice in these experiments he was well trained in similar investigations in previous years.

Each subject first submitted to two series of experiments of ten flexor and ten extensor movements each in which the tension of the spring in the balance "*F*," Fig. 2, was reduced to 0. He was urged to make the quickest possible movement while at the same time avoiding back pressure. Following this were two series in which for each subject the tension of the spring was set at 200. In these series the subjects were not requested to avoid back pressure without giving attention to the initial stage of the movement. It is probable that the lack of such instruction, since these series followed immediately after the preceding, suggested the backward push so vividly that there is a greater difference between the back pressures with no tension and those with a tension equivalent to 200 g. than would otherwise appear. The results of these experiments appear in Table IX in which the time of move-

TABLE IX.

| Subj. | Tension of Spring. | Extension. | | Flexion. | |
|---|---|---|---|---|---|
| | | Time. | Pressure. | Time. | Pressure. |
| S. | 0 | 94.5 | 93.12 | 86.9 | 97.5 |
| | 200 | 76.6 | 946.8 | 64.9 | 1336.6 |
| T. | 0 | 80.6 | 41.25 | 69.1 | 226.5 |
| | 200 | 76.3 | 818.4 | 65.0 | 1031.2 |

ment and back pressure are arranged horizontally in line with the figures which designate the tension of the spring. In every case the greater back pressure is in the flexor movements where also the time is the shorter. When the tension of the spring was 0. and the subjects were asked to avoid back pressure, at the same time making the quickest possible movement, some back pressure was, nevertheless, exerted. In the case of S. it was 93.12 g. and 97.5 g. in extension and flexion respectively. For T. it was 41.25 g. and 226.5 g. respectively. When the tension of the spring was made equivalent to 200 g. and the subjects were not instructed to avoid back pressure but only to make the quickest possible movement, S. considerably reduced his time of movement; namely, $94.5\sigma$ to $76.6\sigma$ in extensor movements. T.'s time was less reduced; from $80.6\sigma$ to $76.3\sigma$ in extension and from $69.1\sigma$ to $65\sigma$ in flexion. When the tension of the spring was made 200 g. and the subjects attended only to the speed of the required movement, S.'s back pressure was increased from 93.12 g. to 946.8 g. in extension, and from 97.5 g. to 1,336.6 g. in flexion, while T. increased his back pressure from 41.25 g. to 818.4 g. and from 226.5 g. to 1,031.2 g. in extension and flexion respectively. These results indicate that these subjects have not in the course of practice been trained out of the backward start or back pressure. They indicate, further, that attention when directed to the beginning of a movement increases its time, for when under the second condition of the experiment, namely, when they were required to attend only to the speed of the movement and not to the behavior at the beginning of the movement, the time of movement is reduced, as pointed out above, from $94.5\sigma$ to $76.6\sigma$, and from $80.6\sigma$ to $76.3\sigma$ in extensor for S. and T. respectively, and in flexor movements from $86.9\sigma$ to $64.9\sigma$ and from $69.1\sigma$ to $65\sigma$.

## SUMMARY.

The results of this investigation may be briefly summarized. With but two indecisive exceptions the fourteen subjects of

these experiments are more rapid in flxeor than in extensor movements. But while the maximal flexor rate is the higher it is less constantly maintained than that of extensor movements; *i. e.* the mean variation of the flexor times is higher than that of the extensor times.

The rate increases and the mean variation decreases as the running start is made longer but not beyond a certain maximal start. Among starts of 0.5 cm., 1 cm., 3 cm. and 7.5 cm. that of 3 cm. is most favorable for a rapid movement over a course of 50 cm. The greatest loss in time and the largest variations occur within the first centimeter of the course; perhaps within the first half centimeter.

For some subjects there is, as a result of practice, a greater increase in the rate of extensor than of flexor movements.

The initial backward start or "back pressure" is not a constant factor in determining the rate of movement. The greater amount of "back pressure" almost invariably occurs, on the average, in preparation for flexor, which are the more rapid movements.

The duration of "back pressure," as far as this investigation shows, has no effect upon the rate of movement.

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# LITERATURE.

## RECENT STUDIES OF NORMAL ILLUSIONS OF MEMORY.

Analysis of the memory consciousness reveals a group of organic sensations and affective processes as the characteristic mark of familiarity in certain parts of our experience. These constitute the added symbol that connects them definitely with our own past. How efficient is this symbol, and what are the ways in which it may fail to fulfill its duty, we may ask, for nature has made few things perfect, and our memory is no exception to this rule. False memory, even more than false perception, is no useful function of mind. Yet, like the illusions of perception, the illusions of memory must be regarded as a result of normal functions at work under somewhat unusual conditions.

In the review of the literature given below I shall not follow Titchener's inclusive use of the term 'memory illusion,'[1] but shall limit the consideration to *paramnesia* as a normal process, that is, as it occurs in every day life. Taken in this limited sense, memory illusion is not a topic that has yet received very much attention in the text books, and general treatises on psychology, though special articles on the subject have not been infrequent.[2] This neglect is the more remarkable in view of the fact that false memory is a matter of relatively frequent occurrence and is certainly of great practical importance. There are also no great difficulties in the way of its investigation. Experiment has not, however, been seriously applied to the study of it until within the last five years. Of the studies that have been made within that time I shall take up first merely the quantitative aspect of normal memory illusion, after that some of the conditions upon which memory illusion depends. Our first question is: To what degree can our memory generally be depended upon? To what extent is our subjective certainty a measure of objective reality? We may consider this question, first, independently of the conditions under which the memory illusions occur. We might suppose on the one hand that the ability to tell whether one really remembers a thing or not could not depend upon anything but upon the degree with which one tries (or on other characteristics of the person remembering) and had nothing to do with the things remembered or other external conditions. And, on the other hand, we might suppose that a real memory illusion, analogous to the illusions of perception, would depend more upon the external condition of the things to be remembered than upon the person trying to remember. These are two things that should be kept in mind in considering the results that are regarded as instances of memory illusion.

Some of the studies on the amount of forgetting under different conditions give incidentally something on the amount of memory illusion. I shall describe, first, very briefly, several studies in which relatively simple material and short time intervals were used, and then gather the results together in one table. The same will then be done for the more complex material and longer time intervals. Whipple in his

[1] Outline of Psychology, 1896, p. 285.

[2] For one of the earlier ones see Burnham's Study of Memory, this *Journal*, Vol. II, 1889, pp. 431-464.

study on the memory for the pitch of tones gives in one of his tables the right and wrong cases in two groups—the judgments made with certainty and those made with uncertainty as to correctness.[1] Simmons presented to his subjects a series of colors, each color being followed by a numeral. After a certain time interval the series of colors was again shown slowly while the subject was requested to record for each color the first numeral that occurred to him, stating also whether he regarded that numeral as the correct one, the wrong one, or whether he was doubtful.[2] Finzi presented visually series of numerals, nonsense syllables, and words to his subjects and after time intervals of from two to thirty seconds asked the subject to try to recall the series. He gives the results on the amount of memory error.[3] Diehl exposed groups of numerals, lines in different positions, and colors for ten seconds at a time and had the subject recall the groups after 24 hours.[4] These four studies give the following results on the amount of memory error:

| Wrong. | Right. | | |
|---|---|---|---|
| 26.6 % | 73.4 % | Certain | Whipple |
| 48.9 " | 51.0 " | Uncertain | Whipple |
| 47.0 " | 52.9 " | Simmons | |
| 21.0 " | 76.5 " | Finzi | |
| 11.6 " | 80.6 " | Diehl | |
| 36.6 " | | | |

This gives a general average of 36.6% wrong. That is to say, in 36% of the instances when one reports a thing from memory and thinks he is correct, he is mistaken. But on account of the great variations this average means comparatively little. Perhaps most significant among these figures is the 26.6% wrong in Whipple's results when the subjects were 'certain' that their judgments had been correct. When they were doubtful but still thought they were correct they were wrong about half the time, 48.9% of the number of instances. Some indication is also given of the effect of the nature of the material upon memory illusion. The simpler material seems to be more conducive to memory illusion.

The conditions in the studies just considered are found only in the laboratory. Most of the recent studies on memory illusion and its related problems have been made with more complex materials than these and with longer time intervals. The aim has been to approach more nearly the usual conditions of every day life. The interest of the investigators has always been the practical one. How far can we depend upon our memory for accuracy under the conditions in which we commonly use it? The studies that come under this topic may be divided into three groups, according to the material used. (1) A complex picture has been shown the subject, and the same described from memory after different time intervals. (2) Subjects have been asked to describe from memory a place they have seen once (or many times), not knowing beforehand that they would be called upon to do so. (3) Subjects have been asked to describe an event which they had witnessed, generally without knowing that they would be called upon to do so Not all of these, however, give a quantitative statement of the degree of memory illusion, being concerned with other related questions.

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[1] *Am. Jour. of Psych.*, 1901, p. 421.
[2] Prevalence of Paramnesia. *Psy. Rev.*, 1894.
[3] Zur Untersuchung der Auffassungsfahigkeit und Merkfahigkeit. *Psychol. Arbeiten*, III, 1901.
[4] Zum Studium der Merkfahigkeit. Experimentelle-psychologische Untersuchung, Berlin, 1902.

Lobsien used a picture of a country scene, with a small boy fishing in a brook in the foreground, and some buildings, trees and a bridge, etc., further back. This was shown for two minutes to school children, aged 9-14 years. Immediately afterwards they were asked to answer twelve questions concerning the picture. On three of these questions he gives the results concerning the matter in which we are now interested.[1] Wreschner used a picture of an old man holding a bowl of food on his lap, from which he is feeding a small boy standing by his side. This was shown to adult subjects for forty-five seconds. After different time intervals, varying from nineteen hours to seventeen days, these subjects answered a list of questions concerning the picture. Some of them without seeing the picture a second time described it in this way more than once.[2] Stern showed a colored picture to school children, aged 10-12 years, for one minute. The picture was that of a "Bauernstube," and more complex than the others so far mentioned. Immediately after being shown the picture, the children described it from memory in as full detail as they could, first without the aid of questions. When this had been done they answered in addition a list of supplementary questions concerning the picture.[3] Borst used five different pictures: 1. Picture of three children playing with a sheep in a meadow. 2. Picture of a rabbit. 3. Picture of three children in a garden. 4. Picture of a woman and two children looking for mushrooms. 5. A shepherd with a child and dog, driving some sheep through the woods. Each of these was shown for one minute to adult subjects. After time intervals of three, and nine days the subjects described them, first, without questions, and then immediately afterwards answered a list of questions concerning them. The quantitative results from these studies may be brought together in general averages, disregarding, for the time being, the differences in the conditions under which these results were obtained. The average percentages of wrong statements made by the subjects when they thought they were right are as follows:

| Wrong. | | |
|---|---|---|
| 89.0 % | No. in each class of things in picture, | Lobsien. |
| 89.5 " | Material in roof of house, | |
| 32.0 " | Material in bridge, | |
| 26.0 " | | Wreschner. |
| 22.0 " | | Stern. |
| 14.3 " | | Borst. |

Of these figures the very high percentages from Lobsien's results bring out a matter to be considered with more detail later. They are percentages of the number of wrong statements in answer to particular questions, and the questions refer to things in the picture that the subjects did not particularly attend to.

Two other studies in which the subjects described from memory a place they had seen several times, without knowing that they would be called upon to do so, may also be considered at this point. Lipmann asked his students to describe a lecture room (in which they had met ten times) on the third day after they had ceased to meet there. They described it first spontaneously and then answered a list of questions.[4] Stern did the same with his class eight days after they had met in the

[1] Aussage und Wirklichkeit bei Schulkindern. Einige experimentelle Beobachtungen. *Beiträge z. Psychol. d. Aussage*, Vol. I, 1903, pp. 158-221.
[2] Zur Psychologie der Aussage. *Archiv f. Gesamte Psychol.*, I, 1903, pp. 148-183.
[3] Die Aussage als geistige Leistung und als Verhörsprodukt. *Beiträge z. Psychol. d. Aussage*. 1904, pp. 269-415.
[4] Experimentelle Aussagen über einen Vorgang und eine Lokalität. *Beiträge z. Psychol. d. Aussage*, I, 1903, pp. 222-231.

lecture room in question. The description was made this time, only in answer to questions.[1] For these two studies the degree of memory illusion is: Lipmann—27% wrong; Stern—18.9% wrong.

Lipmann and Stern also had their students describe an event, which took place in the lecture room, some time after its occurrence—an event which at the time of its occurrence they had not known they would be called upon to describe. Lipmann's 'event' was as follows: "At about 9.45, during my lecture, there was a knock at the door of the lecture room. I called, 'Come in,' and a girl entered and whispered to me. She is small, has brown hair and eyes, wears a large, black felt hat, black jacket with double row of buttons, black skirt, black shoes and gloves, and carries an umbrella. I answer her: 'I have no time now; you may wait for me, I shall be through soon.' 'Then I may wait here?' 'I don't care. Take a seat there, but keep quiet.' She takes a book from the lecturer's desk, sits down and reads until 9.55, when she exclaims suddenly, 'It is frightfully hot here; may I open the window a little?' and rises and reaches out to open the window. 'Oh no, leave it now. It is not that hot—is it (turning to my class). You may wait outside.' 'It is too warm for me here; I shall wait outside.' She goes out quickly, putting the book into her pocket."[2] Two days after two subjects described this occurrence from memory, again, first spontaneously, and then in answer to questions. Three subjects did the same three days after. Stern used a similar occurrence in a lecture-room for his experiment. During the lecture a student comes in, asks to speak with Stern, hands him a manuscript, and asks permission to consult some books in a bookcase in the room. He goes to the bookcase, takes out a book and reads for five minutes. Then leaves the room, taking the book with him. On leaving, Stern asks him to wait outside until he is through. A week later the students witnessing this event were asked to describe it, first, without questions, and then with them.[3] The results of these studies were: Lipmann—33% wrong; Stern—32% wrong,—all of statements that the subjects thought to be correct.

Let us stop a moment, now, and consider all these results together. They have been obtained under a considerable variety of conditions, as was noted, but, excluding two,—the 48% of wrong judgments obtained by Whipple, when the subjects were *uncertain*, and the 47% wrong of Simmons, in an entirely different kind of experiment—the general averages do not vary very greatly. They agree in fact much more closely than results generally do when the conditions have been so different. Considered as exact quantitative statements of the degree of memory illusion they can have no general significance but they go to show that the average man is likely to be mistaken about one-fourth of the time when he reports a thing from memory, and conscientiously believes that he is speaking the truth. This result is certainly not in harmony with general conceptions of the trustworthiness of memory, and should go far towards shaking pre-conceived notions on the matter. Intelligent and observant men know that there are such things as memory illusions, but I doubt whether many would, from a guess, place the figures nearly so high as those we have cited. The "man on the street," however, does not proceed upon the assumption that there is any such thing as memory illusion. For him, every man knows whether he is speaking the truth or not in matters of memory. He either knows, remembers that a thing is so, which in that case *is* objectively so, or he has forgotten. Stern points out that the naïve

[1] Wirklichkeitsversuche. *Beiträge z. Psychol. d. Aussage*, II, 1904, pp. 1-32.
[2] *Op. cit.*, p. 91.
[3] Wirklichkeitsversuche, p. 15.

mind makes two assumptions. First, that a statement conscientiously made with certainty is objectively correct. And second, that a statement that is not objectively true, is subjectively a lie.[1]

In the further consideration of memory illusion we may take up some of the conditions that have been found to influence its degree. From the data on these we may also infer something as to the nature of the causes of it. In the question as to how much one can remember at all, we should rightly expect, without special investigation, that the amount would depend upon the time intervening, the degree of attention paid at the time of the experience, and perhaps, also, upon the nature of the material. The same would, however, hardly be expected to be true of the degree of memory illusions. We may forget more as time goes on, but why should we remember falsely more? And similarly, as regards the original degree of attention and the other conditions. The facts, however, are again not in accord with *a priori* expectations.

Let us consider results that have been reached with regard to the influence of the passage of time upon the extent of memory illusion. In an expriment made by Henderson[2] short passages were read to the subjects who were then requested, among other things, to write out what they had remembered of this, after two days, and again after four weeks. Each time they were requested to use the same words as in the original, when remembered, and to underscore those which they thought had occurred in the original. In one of Stern's experiments already quoted the subjects described four times, a picture, which they had seen but once, the first time immediately after it had been shown, and then again after intervals of five, fourteen, and twenty-one days. In a similar experiment by Wreschner the picture was described from memory three times after different time intervals, varying with the different subjects. In Borst's study time intervals of three and of nine days were used. Other studies of this question do not give quantitative results on this particular point. Bringing the figures from these several studies together gives the following table for the extent of memory illusion after different time intervals:

| 1st recall. | 2nd recall. | 3rd recall. | 4th recall. | |
|---|---|---|---|---|
| | 27.6% (2 d.) | 28.5% (4 wks.) | | Henderson. |
| 10.5% (3 d.) | 12.1" (9 d.) | | | Borst. |
| 11.5" | 12.5" | 28.1" | | Wreschner. |
| 5.8" (Immediate.) | 7.3" (5 d.) | 10.1" (14 d.) | 12.8% (21 d.) | Stern. |

These figures demonstrate clearly that memory illusion *increases with time* as well as does the amount that we forget entirely. The studies made, do not, however, permit the plotting of a curve for memory illusion, comparable to the curve for forgetting. The interpretation of the fact of this increase in memory illusion with the passage of time may be left until the results with regard to the other conditions influencing the extent of memory illusion have been considered.

A second condition that has been shown to be influential in this way is that of the degrees of attention given to the different parts of the complex experience, and the difference in the nature or aspects of the material itself. These two factors cannot be separated in such studies as those now before us. When a complex picture is presented there are many things to be observed. Left to itself, attention will seize upon the central things and many details will not be noticed at all. The picture itself is so constructed, indeed, that all normal observers will tend to do just this thing. They will vary from this only

[1] Aussagesstudium. *Beiträge z. Psychol. d. Aussage*, I, 1903, p. 48.
[2] A Study of Memory for Connected Trains of Thought. *Psy. Rev. Mon. Suppl.*, 1903.

in the degree in which they have special interests. Furthermore the nature of the material itself varies in different cases over a wide range, showing difference in forms, colors, intensities, numerical and spatial relations. Such differences would account at once for differences in the *amount* of these different things remembered. But it is not equally plain at once that there should be different degrees of memory illusions for these things. If one has not observed a thing closely it is easy to understand why he should be more apt to forget it altogether, but this is different from his more readily remembering it falsely. Some of the numerical results in the difference in the extent of memory illusion for the different things or aspects of things in a complex experience are as follows: In Diehl's study numerals, colors, lines in certain positions, and angles were presented in groups. The percentages of memory illusion for these are:

| Numerals. | Colors. | Lines. | Angles. |
|---|---|---|---|
| 15.3% | 10.% | 6.7% | 14.3% |

Borst who employed several pictures, groups the results under the following categories: (1) Objects and persons. (2) Qualities, excluding color. (3) Colors. (4) Spatial relations. (5) Activities. (6) Numerical relations. For these the percentages of memory illusions are:

| Obj. and persons. | Qualities, excluding colors. | Colors. | Spatial relations. | Activities. | Numerical relations. |
|---|---|---|---|---|---|
| 6.6 | 17.7 | 30.4 | 7.1 | 15.5 | 22.3 |

Wreschner groups his figures on memory illusion under the categories of (1) personal characteristics, and (2) characteristics of things, for the picture of the old man with a bowl of food, feeding a small boy by his side. His percentages are:

| | Grandfather. | Boy. |
|---|---|---|
| Personal characteristics | 30.4% | 43.6% |
| Characteristics of things | 42.7" | 77.2" |

Results from two studies by Stern may be quoted in addition. One concerns the description from memory of a lecture room; the other, that of the complex picture of a peasant's room. In the first case the percentages of memory error in answering a number of questions may be given. These are:

| | Per cent. |
|---|---|
| Appearance of outside of entrance door, | 2 |
| Appearance of ceiling, | 2 |
| Appearance of lecture stand, | 6 |
| Presence of bars before windows, | 13 |
| Appearance of inside of entrance door, | 17 |
| Presence of stove in room, | 21 |
| No. of windows, | 28 |
| Position of door, | 66 |
| Presence of a second door, | 74 |

The second of Stern's studies is that in which he used the complex picture of a peasant's room. The percentages of memory illusion, considering all the statements made, are as follows, for the different things in the picture:

| Man. | Woman. | Boy. | Cradle. | Child in cradle. | Clock. | Pictures on wall. | Doll. | Bed. | Window. | Dog. |
|---|---|---|---|---|---|---|---|---|---|---|
| 0 | 0 | 3.0 | 4.0 | 5.0 | 5.0 | 6.5 | 8.0 | 11.0 | 11.0 | 24.0 |

Grouping the results under different categories, he has:

| Things. | Persons. | Activities and Cond. of persons. | Spatial. | Qualities. | Colors. | Numerical. |
|---|---|---|---|---|---|---|
| 4 | 2 | 10 | .67 | .33 | 17. | 31. |

So far as quantitative differences go, these figures speak for themselves. They show great differences, varying in one of Stern's studies from 2 to 74%. Of the two factors here combined in producing such differences the degree of attention and interest is undoubtedly the more important one. The *central* things are the ones about which statements from memory are most apt to be correct. The minor details that give these a setting are objects of less interest, are not particularly attended to, and, when described from memory, are apt to be wrongly described. Thus in Borst's results the statements about objects and persons in the pictures give the lowest percentage of illusion. In Wreschner's results the statements about characteristics of persons are less frequently wrong than are statements about characteristics of things. In Stern's picture the peasant and his wife are the points of central interest, and statements about them are always correct, while one-fourth of the statements about the dog are wrong.

There is a third general condition that has been found to influence the extent of memory illusion. This is the condition under which a statement is made, *i. e.*, as to whether it is an answer to a question, or is a spontaneous statement on the part of the subject. Several studies give results on this point, and they are all in entire agreement. Lipmann's studies (1) on the description of an event taking place in a lecture room during a lecture, and (2) on the description of a lecture room, gave the following results:

(1) Without questions—10% wrong; with questions—42% wrong.
(2) " " —27" " " " " —27" "

Borst's figures are: without questions, 11% wrong; with questions, 17% wrong. Stern's figures for the description of an event in a lecture room are: Without questions—23% wrong; with questions—49% wrong. For the description of the picture of the peasant's room they are: Without questions—6% wrong; with questions—33% wrong.

These figures, taken together, leave no donbt of the fact that memory illusion is greater when the statements made are answers to particular questions, than when the statements are made spontaneously on the part of the subject without special questioning. Two possible, or partial explanations of this fact should be considered. (1) When a subject describes a thing from memory on his own account, the things most prominent, and best remembered will come to his mind, while others will not be recalled at all, and thus the possibility of making misstatements concerning them will be avoided. (2) Questions are always more or less suggestive in the first place, and they may as frequently suggest the wrong thing as they do the right. They are also as apt to touch upon matters to which one has attended but little and therefore remembers poorly, as they are to touch upon the others. These two factors taken together account readily for the greater tendency to memory illusion when the statements are answers to questions. Both in reality depend upon the degree of attention and interest with which the different elements in a complex experience are originally apperceived.

With this statement of the extent of memory illusion in general, and of its extent as dependent upon special conditions we may turn now to the interpretation of the results, to the question of what the nature of memory illusion really is. It has been seen that in general when the average man reports a thing from memory and conscientiously believes that he is stating the truth, he is, nevertheless, mistaken about one-fourth of the time. And this tendency to false memory is the greater the longer the time since the original experience and the less carefully one has observed. In other words, memory illusion follows the same

laws, in these matters, as does simple forgetting. The suggestion was made at the beginning of this review that misstatements from memory might be due either to genuine memory illusion, or to what may be called carelessness in discrimination in the process of recall. The results on the conditions that influence the extent of memory illusion suggest that what is here called illusion is not always genuinely of that nature. If it were, we should have to say that the tendency for the recognitive elements to get assimilated with the wrong content increased with the time interval, and with the lack of attention and interest with which the experience had been regarded. This might, of course, be the fact always, however strange it might seem in the entire absence of any explanation that we could imagine for it. That it does happen sometimes is borne out by general observation. In all cases, for instance, in which one *frequently* thinks over his experiences he is very apt to come in the end to seem to remember very clearly things of which he was at first very doubtful; and his memory is quite apt to be wrong. This happens through a confusion of what he at first remembered, and what he afterwards often imagined. In other words, he forgets that he has only imagined a thing of which his memory was not certain, and then remembers what he has imagined as though it were a real memory of an actual fact. The habitual liar who finally comes to believe that his stories are true, if there is such, would be an illustration of the point in question. But in all instances in which the experience is not frequently gone over in memory, such a process is eliminated, and the question arises whether in such cases our false memories proceed from real memory illusion, or whether they are due to other causes. That they should result from illusion is not *a priori* probable. For there are no evident reasons why in these instances memory illusion should increase with the time interval, and be the greater the less carefully one has observed. We know that memory imagery is, under these conditions, less complete, less vivid, and more apt to be absent entirely, but this does not account for the wrong imagery being more apt to be taken for the right. The recognitive elements that are assimilated with the central core of our memory imagery should be affected in the same direction by time as is the latter. We know that these are, however, even less affected by time than is the image; for recognition is often possible after the image can no longer be revived. Some light is thrown upon the nature of the whole process by a general, but none the less trustworthy observation, namely, that in attempting to reconstruct an experience from memory it is not really from memory alone that we proceed. We reconstruct such an experience very largely from our knowledge of how things ought to have or must have been.[1] We rely only in part upon our recognition or memory of how they really were. The significant thing about this observation in connection with the present question is the light it throws upon what the general condition of our recognitive memory must be. It indicates at once that the recognitive elements play a much smaller part in what we are accustomed to call memory judgments than is generally supposed to be the case. They are not the only means by which we decide what the facts of our experience have been, perhaps not the main means. And this very strongly favors the view that much of what we call memory illusion is not that at all, but is mistaken judgment, resulting from other sources than memory.

It would be interesting to turn now to a more detailed consideration of what these other influences may be which determine our judgments of past experiences, and which may produce wrong as well as correct

[1] Stratton: Experimental Psychology and Culture, N. Y., 1903, p. 187.

judgments, but little can be said further on the matter. We are left with a few suggestions only, and no well established facts. The question is still one for investigation. In a number of experiments on the memory for weak intensities of sounds it has been found that the second is very frequently judged as more intense than the one just previously given, although objectively they were the same. On the other hand Baldwin and Shaw found that certain sized squares increased in memory so that when the subject was requested to choose a square just equal in size to one previously shown, he chose one that was larger.[1] But Leuba found that for different intensities of light one rule holds true for weak intensities, another rule for greater intensities.[2] We have here two classes of memory illusion that have been differently explained. The old explanation of the fact that of two equal intensities the second is judged the greater, was that the memory image of the first weakened. But Leuba's results contradict such an explanation, and Bentley's[3] and Whipple's[4] studies make it further impossible. Leuba explains his results by the assumption that our memory tends towards 'average experiences.' Weak intensities of light we thus remember as more intense than they really were, and great intensities we remember as weaker than they really were. This explanation brings in a wide principle of interpretation of memory illusion in the memory of any kind of experience. We may substitute for this statement another form and say that we remember things as we have been accustomed to experience them. The unusual, unless this fact has been particularly noted in the original observation, tends, in memory, to take the form of the usual. It is easier for the mind to move in the lines of its habits, and it does so even to the extent of confusing its new with its old experiences. We might say that here, again, where there is a misstatement due to this tendency, we are concerned with a genuine memory illusion; that where we substitute the usual, and habitual for something which differs somewhat from this there are attached to our imagery the recognitive elements that make it a true memory act. But there is no need of such an assumption. It is more probable that the fact is better accounted for by the absence, and inadequacy, generally, of these recognitive elements. That the memory judgment is frequently not made upon the basis of these is indicated by the observation that we so often reconstruct our past experience by reasoning it out. It is because of the absence of the real memory consciousness that these other factors can come in to decide what we call our memory judgments. In our ordinary thinking we have not the power of keen discrimination as to what these judgments are based on. We confuse our rational constructions with our real memories; we reason a thing out and then say that we remember that it was so. Habitual imagery has some of the general characteristics of complete memory imagery in its greater ease of arousal, and arousal in greater detail and vividness. With the lack of close discrimination for these border cases, we take the one for the other: the habitual image that is wrong, for a real memory image that is right.

We have thus suggested two factors that produce misstatements concerning our past experience, of the sort that have been called memory illusions; (1) rational construction, and (2) the tendency for the mind to substitute in the recall of the past the usual and habitual for what has differed slightly from that. Both rest ultimately on the gen-

[1] Memory for Square-Size. *Psych. Rev.*, Vol. II.
[2] A new Instrument for Weber's Law, with Indications of a Law of Sense Memory. *Am. Jour. of Psych.*, Vol. V.
[3] Bentley: The Memory Image and its Qualitative Fidelity, *Am. Jour. of Psych.*, Vol. XI.
[4] Whipple: *Op. cit.*

eral absence and inadequacy of our recognitive consciousness. Considering these main results of the studies on normal memory illusion, it might be well to turn our attention in conclusion to the conception of memory that they give in contrast with the older and still common view of memory images as 're-incarnations' of past experiences, as 'faded copies' of original perceptions. This comparison needs only to be suggested to show the long way we have gone from that older view. The errors and short comings of the latter are patent in the light of even the few established results already reached. The fewness of these results, and at the same time the influence they are having on our psychology of memory, point to a large and promising field of inquiry, a field full also of practical implications of great importance.

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*Atlas of the Nervous System: including an epitome of the anatomy, pathology and treatment.* By C. JAKOB, with a preface by A. von Struempell. Translation from the second German edition; edited by E. D. Fisher. W. B. Saunders & Co., Philadelphia and London; 1901. pp. 218. (112 colored lithographic figures and 139 other illustrations.)

I am glad of the opportunity to recommend this little book to my colleagues of psychology who are obliged, from time to time, to have recourse to text-books of neurology. The work has its obvious limitations, both of space and of time; it covers an immense field in small compass, and the German original, from which the translation has been made, dates back to 1899. Despite these drawbacks, it is one of the most useful and reliable compendia with which I am acquainted.

E. B. T.

*Willensfreiheit und wahre Freiheit: mit einem Anhang über den heutigen Stand der Frage vom freien Willen*, von G. TORRES, München, 1904. pp. 46.

This is a sensible essay upon the eternal problem of human freedom, in which the author draws a sharp distinction between philosophical or causal and practical or relative freedom, and discusses in detail the questions of merit and responsibility from the standpoint of determinism. The book is written on a moral basis and in ethical terms; but its underlying psychology is sound, and its positions can readily be translated into psychological language. P. E. WINTER.

*The Eternal Life*, by HUGO MUENSTERBERG. Houghton, Mifflin & Co., Boston and New York, 1905. pp. 72.

The author sets out from his well-known thesis that the world of science, temporal, spatial, causal, is merely a construction, made for special purposes by the free personality of man. To approach the question of the eternal life, we must "emancipate ourselves from this unnatural view, and apperceive our life as act and not as object, as creator of time and not as a chance occurrence in time" (p. 26). "The real personality, the subject of will and thought, is not an object in time, as it is itself the condition of time. Its whole reality lies in its attitudes and in its acts" (pp. 16 f.). "My real life as a system of interrelated will-attitudes . . . is independent of birth and death; . . . it is immortal; all possible thinkable time is inclosed in it; it is eternal" (p. 27). "We do not desire the tone of this individual life to last beyond its internal, eternal rôle, throughout the symphony of the Absolute; its immortality is its perfect belonging to that whole timeless reality, belonging there through its human relations to its neighbors, and through its ideal relations to the ultimate values" (p. 70). These quotations will give some idea of the writer's standpoint, which,

as it will not satisfy those who desire a personal immortality, so also is beyond the reach of criticism in short compass, implying as it does a definite philosophical system. P. E. WINTER.

*Man's Responsibility; or How and Why the Almighty introduced Evil upon the Earth*, by T. G. CARSON. G. P. Putnam's Sons, New York and London, 1905. pp. v, 524.

An exposition in detail of the plan of the Almighty in the creation of mankind, together with suggestions, derived from the principles involved, for the betterment of government and society. "The design adopted by the Creator evidently was to construct man in the manner which he judged best for him, and so to place him, that he could put to use all the functions and capacities with which he was endowed. I cannot see how it is possible that this plan can strike any one with surprise. . . . Everything that is constructed upon earth is formed of materials, and the materials of which the human mind is formed are principles. But all materials are alike capable of being used for good purposes and for bad. A man who is constructing a house may seize a batten, and with it slay his brother. Are battens therefore to be eschewed, and houses reprobated? Nonsense like this is not entertained when discussing practical affairs. Neither, it is clear, do such considerations weigh with the Almighty" (pp. 351 ff.).

P. E. WINTER.

# BOOK NOTES.

*Socialism: Its Theoretical Basis and Practical Application*, by VICTOR CATHREIN. Translated by Victor F. Gettelmann. Benziger Bros., New York, 1904. pp. 424.

This work, translated from the eighth German edition, has seemed to many to contain the best of all refutations of socialism to be found in the German language. One of its great merits is that it is not only a refutation, but gives an admirable account of the rise and development of these theories in different lands and of their fundamental tenets.

*The Socialization of Humanity*, by CHARLES KENDALL FRANKLIN. Charles H. Kerr & Co., Chicago, 1904. pp. 481.

Here we have a system of monistic philosophy based on an analysis and synthesis of the phenomena of nature, life, mind and society through the law of repetition. The range of the book is extraordinary. It treats the origin of life, the physics and chemistry of the senses, the intellect, emotions and will, animal mechanics, realism and idealism, the expenditure of energy controlled by mind called the fourth, and that controlled by moral sense called the fifth, and that by the social sense called the sixth law of motion. Ethics, religion, social organism, God, are all treated at length in a comprehensive but too general way in this suggestive and venturesome, but, from the printer's point of view, rather unattractive book.

*Art in Theory. An Introduction to the Study of Comparative Æsthetics*, by GEORGE LANSING RAYMOND. G. P. Putnam's Sons, New York, 1904. pp. 286.

This prolific, interesting and suggestive writer here treats of nature and art form and significance as antagonistic as seen in classicism and romanticism, art forms as representative rather than imitative, or communicating thought and feeling and thus involving mind, discusses the art impulse, the higher as distinguished from other representative minds, theories of beauty, taste, its tests, art history, the relations of art to physiology, psychology, etc. The general criticism which is surest to be made of this work is that the author has not put himself sufficiently in possession of the recent theories of really scientific or philosophical thinkers. The paucity of allusion to contemporary French and German writers who have done so much is marked.

*The Genesis of Art-Form. An Essay in Comparative Æsthetics*, by GEORGE LANSING RAYMOND. G. P. Putnam's Sons, New York, 1904. pp. 311.

The writer here seeks to trace to their sources in mind or matter the methods employed in the composition of art forms. He identifies this action of the mind with its action in scientific classification, and has arranged the methods in the logical order of their development. Each principle, too, is amply illustrated, and the many cuts and lithographs of pictures, buildings, and the many poetic extracts, all of which seem to have been made with remarkable sagacity and good

taste, make the book one of extreme interest. The reader will marvel at the author's range of knowledge, for it includes also sculpture, architecture and music. Interesting to the psychologist, too, are the discussions of gradations versus abruptness in transition, of continuity and interspersion, repetition and alternation, symmetry and parallelism, consonants and dissonants, congruity and incongruity and comprehensiveness, grouping, order, balance, subordination and complement.

"*Optical Illusions of Reversible Perspective*," a volume of historical and experimental researches, by J. E. WALLACE WALLIN, Demonstrator in Experimental Psychology at Princeton University. Published by the Author, Princeton, N. J., 1905, pp. 330; profusely illustrated. (Price $2.25 with cloth binding, $1.25 with paper, including 15 cents for postage.)

Dr. Wallin states that this volume was prepared during his connections with Clark, Michigan and Princeton. The work is divided into two parts, and the first or historical part is mainly devoted to geometrical intaglios, concavities and convexities and pseudoscopic outlines. The second or experimental part describes new figures and the author's experimental records. Then comes perspectivity in momentary exposures and correlations. Chapter IX deals with preliminary introspections, distance and size, estimations, growth of visual forms and incidental suggestions. Then follows a chapter on accommodation and the third dimension, the distance equation, white and black rods, fixation and reversion tests. The author then discusses perspectivity in the case of school children, the duration and alternation of perspective reversals, perspective presentations and practices, and finally gives a recapitulation, dealing with theories, psychophysical and psychological. The work is provided with an excellent index.

Few topics in the whole field of physiological optics are now more difficult or complex than that which deals with illusions. Indeed, many of the most profound questions, not only of psychology but of philosophy itself, and of the entire relation between experience and sense perception, have here their only experimental way of approach. It was high time that this whole subject should be gone over anew, all that has been previously done résuméd, its various sections knit together, and more general conclusions drawn. Dr. Wallin is an indefatigable worker, and has made this a true labor of love to which his spare time for years has been devoted. It is certainly a lamentable comment upon our facilities for publication, rapidly as they have developed during the last decade or two, that there was no agency which could come to his aid, and he was compelled to bear the expense of printing such a volume as this from the rather meagre salary of a demonstrator. While the work itself is worthy of very high praise, and is one that every future investigator must have constantly at hand, the production and publication of the work also shows high moral qualities and most commendable academic ideals. We hope to be able later to present a more comprehensive and detailed review of this work, which is so unique in the circumstances of its publication.

*The Philosophers and the French Revolution*, by P. A. WADIA. Swan Sonnenschein & Co., Ltd., London, 1904. pp. 131.

This small treatise is addressed to the question how far the eighteenth century writers in France can be made directly or indirectly responsible for the outbreak of the French Revolution, however we understand it. The halting attitude between two extremes has often been dubbed historic impartiality. The author's attitude is very emphatic. He insists that no republican party existed in France under

the ancient regime. The philosophers, he thinks, are a standing evidence of the continuity of European history. Revolutionary agitation is a peculiar trait of French history in general, and French literature is a reflection of French society. The principles of 1789 were both destructive and constructive and of both these tendencies the philosophers of the ancient regime were the exponents. They were prophets and preachers of the new gospel, priests of the genius of the French nobility. Perhaps their style was too popular and facilitated the rapid spread of revolutionary ideas. The great majority of the Girondists would have carried out the principles of Emile if they could. We have the frightful logic of DeSade reflected in the croakings of Marat, but the atrocities and terror would still have been there, and the sovereignty of the illiterate mob would have been asserted and anarchy no whit mitigated if Rousseau had not written, and the Revolution was inevitable and would have occurred had the philosophers never written. Indeed, much that Rousseau wrote was irreconcilably opposed to the Revolution. The French philosophers did not cause, but only manifested its principles.

*The Elements of Psychology*, by EDWARD L. THORNDIKE. A. G. Seiler, New York, 1905. pp. 351.

This author is a disciple of Professor James, who states in his brief introduction that his pupil's book will introduce him to more readers than conversely. The introduction takes a general view of the subject matter and problems of psychology. Part first is descriptive and takes up at the outset the feelings of things and qualities as present, then as absent and represented in images and memories; then feelings of facts of personal conditions or emotions, the feelings of willing, general characteristics of mental states, and their functions. Part second treats of the physiological basis of mental life, the nervous system, its action. Part third is dynamic and traces the tendencies to connections, the law of association, dissociation, connection between stimuli, between one mental state and another, between mental states and acts, etc. Movements, selective processes, and the relations of psychology conclude the work. As an appendix we have topics for special study, indexes of illustrations, experiments, names of topics, etc. We have no space to review this book in detail, but we hope to do so later. The specialist will find little that is new in it. It is a text-book from start to finish, seeks to keep close to facts and experiments, lays considerable stress upon the nervous system and delusions, has various exercises and references on the successive topics treated.

*Racial Supremacy*, Being Studies in Imperialism, by JOHN D. GODARD. Simpkin, Marshall & Co., Ltd., London, 1905. pp. 323.

Empire is the predominance of race. The author first discusses imperialism, its nature and products, its demoralizing influences. It is the bane of subject races and inimical to freedom. Liberalism and imperialism are then treated, and next comes commercialism and imperialism, ecclesiasticism and imperialism, the ethics of empire and the burden of empire. These are the leading topics. The style of the writer is rather diffuse and can be sufficiently indicated by the fact that the volume had its origin in a series of articles contributed to the *Westminster Review*. The author pleads for autonomy, liberty, and brotherhood; and insists that both egoism and altruism alike bid us to abjure the doctrine of racial supremacy. In dictating to others, we ourselves succumb to a dictator. Empire means decay. If we insist on it we are travelling the road, however slow and broad it may be, that certainly leads to decay and destruction.

*A Practical Course of Instruction in Personal Magnetism, Telepathy and Hypnotism*, by GEORGE WHITE. E. P. Dutton & Co., New York, 1905. pp. 284.

This work points out to those who have hitherto passed by the subject with contempt and indifference the great powers of the mind which lie within reach of us all—powers the possession of which will enable any man at once to display himself as above his fellows. The first book deals with the power of personal magnetism as an attractive force, and describes the control of breath, concentration, correlation, of habits, demeanor and daily life, which the true hypnotist should possess. The second book deals with mind reading and telepathy. The third gives a concise course of instruction in hypnotism whereby the student can acquire the mastery of that science with all its tremendous power of good. There are three chief methods of hypnotising. The author dwells also on its value in the correction of children, and deems its therapeutic effect very important. He has evidently portrayed about all the work of the Bertillon school. He believes he can cure people addicted to drink and other habits, and is a believer in clairvoyance.

*Les Lois de l'Ergographie. Étude Physiologique et Mathématique*, par MLLE. J. IOTEKYO. Extrait des Bulletins de l'Académie Royale de Belgique. No. 5, Mai, 1904. Hayez, Bruxelles, 1904. pp. 557-726.

The writer first gives a sketch of the first efforts to make mathematical determinations in this field. She then treats the general equations of the fatigue curves with the special relation of the quotient of fatigue. The true mathematical characteristics of these curves, the physiological signification of constants or parameters is considered. She then discusses the effects of alcohol, sugar, anemia of the arm, caffein, experiments on the right and left hand, accumulations of fatigue, and finally gives a plan of new researches.

*The Logic of Human Character*, by CHARLES J. WHITBY. Macmillan & Co., Ltd., London, 1905. pp. 226.

The assumption underlying this essay is the correlation of physical and psychic function. The study of character need not await the completion of cerebral physiology which is not in the embryonic stage. The first chapter treats psychological elements of sensation, perception, automatism, emotion, reflection, mimesis, disposition, judgment and volition. The second treats of the immediate category, implicit character, and the logic of custom, describes pride, admiration, obedience, sympathy, toleration, respect and convention. The third is devoted to the category of the logic of duty; fourth, to that of action; fifth, to piety; sixth, to freedom; seventh, to creation; and the final summary chapter treats of the integration of character, moral progress and educational methods. Each chapter deals with nine special topics, categories or faculties. The work is unique in its schematic nature and the author has certainly worked his way to a unique conception of his theme.

*Recherches cliniques et thérapeutiques sur l'épilepsie, l'hystérie et l'idiotie*, par BOURNEVILLE. Compte-rendu du service des enfants idiots, épileptique et arrières de Bicêtre pendant l'année 1903. Publications du Progrès Médical. Paris, 1904. pp. 346.

Bourneville first describes the history of his institution at Bicêtre during the year 1902. This involves a careful study of the professional education movement, of the population, etc. The second part is devoted to therapeutic, clinical, pathological data. First come several

chapters on nanism of various types. Then follow the different forms of idiocy, imbecility, with and without alcoholism, cranial malformation, epilepsy, nutritive disturbances, synostosis, asymmetry of development, etc.

*Psychological Review*, edited by J. M. Baldwin and H. C. Warren. Monograph Supplements, Vol. VII, No. 1, March, 1905. Yale Psychological Studies, New Series, Vol. I, No. 1, edited by C. H. Judd. The Macmillan Co., New York. pp. 226.

This contains articles by C. H. Judd on Eye Movements studied by means of Kinetoscopic Photographs; by C. N. MacAllister on Fixation Points in a Visual Field; by E. H. Cameron, the Poggendorff Illusion; by Judd and Courten on Zoellner's Illusion; by MacAllister and Steele, Analysis of Reaction Movements; Judd, Practice without Knowledge of Results; Judd, Movement and Consciousness; Judd, Mueller-Lyer Illusion.

*Hume: The Relation of the Treatise of Human Nature* (Book I) *to the Inquiry Concerning Human Understanding*, by W. B. ELKIN. The Macmillan Co., New York, 1904. pp. 330.

This is a careful study as a dissertation for the doctorate at Cornell. After introductory remarks concerning the wider relationships, the author passes on to discuss Hume's aim, subject matter, and method; then his views on perception, their nature and cause, association, space and time, theory of knowledge, cause, belief, probability, necessity, reason in animals, material substance, spiritual substance, miracles, etc. The work concludes with a summary chapter and a bibliography.

*The Metaphysical System of Hobbes*, by MARY WHITON CALKINS. (Religion of Science Library. No. 57.) Open Court Publishing Co., Chicago, 1905. pp. 187.

Miss Calkins has given us an interesting presentation of the Malmesbury philosopher. The first part is logic; the second, the first grounds of philosophy; the third, motions and magnitude; the fourth, the phenomena of nature. Then comes his doctrine concerning unreality of consciousness and the nature of Spirit and of God. The editorial work is well done, and the price (40c. in paper) is reasonable.

*The Native Tribes of Central Australia*, by BALDWIN SPENCER and F. J. GILLEN. Macmillan & Co., Ltd., London, 1899. pp. 671.

This great work, which is copiously illustrated, describes the social organization of these remarkable tribes; ceremonies connected with marriage; the totems; the bull roarers; the ceremonies of initiation and intichiuma; the origin of ancestors; knocking out the teeth; blood letting, giving and drinking; hair; childbirth; food, restriction and cannibalism; customs of burial and mourning; spirit, individuals; the making and power of medicine men; the office of music; methods of obtaining wives; myths of sun, moon and eclipses; clothing; weapons; implements; and decorative art; the names of natives; glossary of terms and tables of measurement.

*The Aftermath of Slavery. A Study of the Condition and Environment of the American Negro*, by WILLIAM A. SINCLAIR. With an introduction by Thomas Wentworth Higginson. Small, Maynard & Co., Boston, 1905. pp. 358.

This is a striking work by a former slave, with a no less striking introduction by T. W. Higginson. It should be carefully read by all those who are influenced by the recent southern literature upon the subject. It treats of the history of abolition, reconstruction, and the southern black code, the southern position as to reconstruction, the

war on negro suffrage, the false alarm of negro domination, the negro in politics, the negro in the law, the rise and achievements of the colored race, the national duty of the negro.

*Le Méchamisme des Émotions* (Leçons faites à l'université nouvelle de Bruxelles, 1903), par PAUL SOLLIER. Bibliothèque de Philosophie Contemporaine. Felix Alcan, Paris, 1905. pp. 303.

This book rejects generally the peripheral theory of the origin of emotion, also the intellectualist theories. It is a purely cerebral theory which is necessitated if one takes a complete view of all the very many aspects of emotion. Emotivity is generated within. The centres that control circulation and nutrition have much to do with it, so that it may in general be called, "cenesthesia cerebrale." The many forms of reaction are the different emotions, but cerebral phenomena is all.

*Die Anfänge des Menschlichen Geistes*, von JULIUS DONATH. Ferdinand Enke, Stuttgart, 1898. pp. 47.

Dr. Donath holds that, however low the psychic state of savages may be, it is usually conditioned by external circumstances, particularly want of practice. Their brain organization permits a development that is scarcely less than that of the average European. Their culture is on the average far higher, and ours far lower than is usually thought. There are really no savages if this implies absence of social order and the elements of speech. The average brain weight of many lower races exceeds that of European woman, and that of the native of Terra Del Fuego materially exceeds that of European man. Karl v. d. Steinen, in his great work, found that materials for color were far older than the necessity of distinguishing them by name, that color-blindness is not found among primitive races, although blue and green are often designated by the same word. Savages, too, have the principle of abstraction involved in counting, but they have little need of numbering things, and hence plurals are rare. So their defective arithmetic implies mental defect only because of limited economic conditions. Primitive people have their own modesty, but no private parts.

*Locke's Essay Concerning Human Understanding*. Books II and IV. (With omissions Selected by Mary Whiton Calkins.) (Religion of Science Library, No. 58). Open Court Publishing Co., Chicago, 1905. pp. 342.

This condensation attempts to present in inexpensive form (50c. paper) the essentials of Locke's teaching in metaphysics and psychology. The first book is omitted because the innate idea question is dead. Book three is omitted because it deals with logic and history.

*Control of Heredity. A Study of the Genesis of Evolution and Degeneracy*, by CASPER L. REDFIELD. Alfred C. Clark, Chicago, 1903. pp. 343.

*Pubescence. A Preliminary Report*, by C. WARD CRAMPTON. Reprinted from the American Anthropologist (N. S.), Vol. 6, No. 5, Oct.-Dec., 1904. New Era Printing Co., Lancaster, Pa., 1904. pp. 705-709.

*Ueber das Unbewusste Denken und das Gedankensehen*, von ALBERT ADAMKIEWICZ. Wilhelm Braumüller, Leipzig, 1904. pp. 64.

*The Vitality and Organization of Protoplasm*, by EDMUND MONTGOMERY. Gammel-Statesman Publishing Co., Austin, Texas, 1904. pp. 82.

*Contribution à l'Étude de la Moelle Epinière*, par R. VARELA DE LA IGLESIA. Richard Fé, Madrid, 1904. pp. 102.

*Thirty Years in Madagascar*, by T. T. MATTHEWS. A. C. Armstrong & Son, New York, 1904. pp. 384.

*Studies in the Psychology of Sex. Sexual Selection in Man.* By HAVELOCK ELLIS. F. A. Davis Co., Philadelphia, 1905. pp. 270.

*Wunder und Wissenschaft. Eine Kritik und Erklärung der okkulten Phänomene*, von RICHARD HENNIG. Ernst Schultze, Hamburg, 1904. pp. 247.

*Aequanimitas, with other addresses to medical students, nurses and practitioners of medicine*, by WILLIAM OSLER. P. Blakiston's Son & Co., Philadelphia, 1904. pp. 389.

*The Mission of Philosophy*, by GEORGE TRUMBULL LADD. Proceedings of the Fourth Meeting of the American Philosophical Association, University of Pennsylvania, Philadelphia, December 28, 29 and 30, 1904. Philosophical Review, Vol. XIV, No. 2, March, 1905, pp. 113-194.

*Ueber den Widerspruch im Wahrheitsbegriffe in Lockes Erkenntnislehre*, von M. ROWENA MORSE. Anton Kämpfe, Jena, 1904. pp. 50.

*Psychological Review*, edited by J. M. Baldwin and H. C. Warren. Monograph Supplements, Vol. VI, No. 5, March, 1905. University of Iowa Studies in Psychology, No. 4. Macmillan Co., New York. pp. 118.

*The Cycle of Life According to Modern Science*, by C. W. SALEEBY. Harper Bros., New York, 1904. pp. 343.

*The Secret of the Circle and the Square*, by J. C. WILLMON. McBride Press, Los Angeles, Cal. pp. 30.

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*Abhandlungen der Fries' schen Schule. Neue Folge. Herausgegeben* von GERHARD HASSENBERG, KARL KAISER und LEONARD NELSON. Erstes Heft. Göttingen, Vandenhoeck and Ruprecht. Price, 4m.

*La Philosophie naturelle intégrale et les rudiments des sciences exactes,* par le DR. A. RIST. Première partie. Paris, A. Hermann, 1904.

*Proceedings of the Society for Psychical Research.* Pt. XLIX, Oct. 1904.

*Grundlinien einer Psychologie der Hysterie,* von WILLY HELLPACH. Leipzig, Wilhelm Engelmann, 1904. pp. viii + 502. Price, paper, 9 m., bound, 10 m.

*Untersuchungen zur Gegenstandstheorie und Psychologie, mit Unterstützung des k. k. Ministeriums für Kultus und Unterricht in Wien, herausgegeben* von A. MEINONG. Leipzig, Verlag von Johann Ambrosius Barth, 1904.

*Psychology: An introductory study of the structure and functions of human consciousness,* by JAMES ROWLAND ANGELL. New York, Henry Holt and Co., 1904.

*Art in Theory: An introduction to the study of comparative æsthetics,* by GEORGE LANSING RAYMOND. Second edition, revised. New York, G. P. Putnam's Sons, 1904.

*The Genesis of Art-Form: An essay in comparative æsthetics,* by GEORGE LANSING RAYMOND. Second edition, revised. New York, G. P. Putnam's Sons, 1904.

*Sociological Papers,* by FRANCIS GALTON, E. WESTERMARCK, P. GEDDES, E. DURKHEIM, HAROLD H. MANN and V. V. BRAMFORD, with an introductory address by James Bryce. London, The Macmillan Co., 1905.

*Du Mode de transmission des idées, conception matérialiste de l' intelligence humaine,* par le DR. L. LEFÉVRE. pp. 51. Bruxelles, P. Weissenbruch, 1905.

*The Classics and Modern Training,* by SIDNEY G. ASHMORE. pp. vi + 159. New York and London, G. P. Putnam's Sons, 1905.

*Studies in the Psychology of Sex—Sexual Selection in Man.* 1, Touch. 2, Smell. 3, Hearing. 4, Vision. By HAVELOCK ELLIS. 6⅜ x 8⅞ inches. Pages xii-270. Extra cloth, $2.00, net. Sold only by subscription to physicians, lawyers, and scientists. F. A. Davis Company, Publishers, 1914-16 Cherry Street, Philadelphia.

*Recherches cliniques et thérapeutiques sur l'épilepsie, l'hystérie et l'idiotie,* par BOURNEVILLE, avec la collaboration de Boyer, Izou, Lemaire, Maugeret, Noir et Boncour. pp. clxxiv+346. Price, 7 f. Paris, Alcan, 1904.

*Sonderschulen für hervorsagend Befähigte,* von PETZOLD. Leipzig, B. G. Teubner and Th. Hofmann. Price, 1 m.

*Yale Psychological Studies. New Series*, Vol. I, No. 1. Edited by Charles H. Judd.

*The Comparison of Animal Magnetism or Hypnotism with the Yoga System of the Hindus*, by C. THAMO THARAM PILLAY, Medical Officer. Colombo, 1897. pp. xix+201.

*The Secrets of the Circle and Trisection of Angles*, by JEREMY CARLISLE WILLMON. Author's edition, McBride Press, Los Angeles, Cal.

*Reprints of Papers*, by CHARLES L. DANA: On Eye Strain and Psychoses; Cerebellar Seizure; Partial Passing of Neurasthenia; Presidential Address; Psychiatry in its relations to other sciences.

*The Logic of Human Character*, by CHARLES J. WHITLEY. London, The Macmillan Co., 1905.

*Psychologie und Pathologie der Vorstellung Beiträge zur Grundlegung der Æsthetik*, von RICHARD WALLASCHEK. Leipzig, Johann Ambrosius Barth, 1905. pp. x+323. Price, paper, 8m, bound, 9m.

*Beiträge zur Psychologie der Aussage, herausgegeben* von L. WILLIAM STERN. Zweite Folge, Zweites Heft. Leipzig, Johann Ambrosius Barth, 1905. pp. 154.

*Schriften des Gesellschaft für Psychologische Forschung*. Heft 15, (III Sammlung) Psychologische Factoren des modernen Zeitgeistes, von Baerwold; Die Bedentung des Urteils für die Auffossung, von MÖLLER. Leipzig, J. A. Barth, 1905.

*Psychological Medicine*, by MAURICE CRAIG. Philadelphia, P. Blakiston's Son & Co., 1905. Price, $4.00 net.

*Theory of Cosmic Reason*, by I. VINOGRADOFF. St. Petersburg, 3d enlarged edition. Published by the author.

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ERRATUM.

On page 228 line 23 for "0.94" read "0.04."